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# ANTIQUITY

A Quarterly Review of Archaeology

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*Edited by O. G. S. Crawford, F.S.A.*

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VOLUME IX

1935

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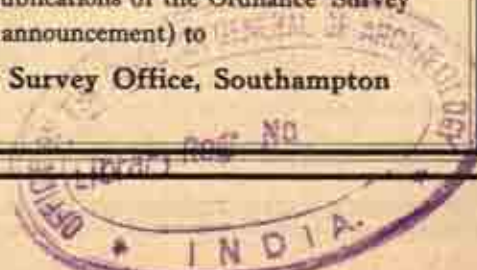
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## PLATE I

PAINTED LIMESTONE  
STATUE, LIFE-SIZE, OF  
PRINCESS NEFERT,  
FOUND IN ONE  
OF THE MASTABAS  
NEAR THE MEYDUM  
PYRAMID (See p. 11)

*Service des Antiquités,  
Cairo*



# Antiquity

## A Quarterly Review of Archaeology

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VOL. IX No. 33

MARCH 1935

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### Editorial Notes

DR C. L. WOOLLEY'S article in the present issue will serve to call attention to an archaeological scandal. Ever since he first discovered an important gold object in 1927 the Arabic Press of Iraq, by innuendo and direct statements, has persuaded people in Baghdad and elsewhere that their country, with the connivance of the Director of Antiquities, was being robbed of its antiquities by excavators. This propaganda has been generally accepted by officials who ought to know much better; and men in high political positions in Iraq have been heard to accuse the British of retaining gold objects which were actually on exhibition in the Baghdad Museum.



During the years 1932-1933 a personal campaign has been conducted against the present Director of Antiquities and the staffs of Foreign Expeditions for unworthy, probably personal, motives. This campaign has been ignored as it deserves to be; but it is now time that the Administration of Antiquities, set up by Miss Gertrude Bell under the British Mandate, and the conduct of the recent Directors of Antiquities, should be justified. Dr Woolley's statement will convince



## ANTIQUITY

archaeologists that the excavations at Ur have enriched the Iraqi nation by securing for it, with all the aid that modern science can afford, a very valuable collection of antiquities, more than equal to the collections from Ur in the British Museum and the University Museum of Pennsylvania at Philadelphia put together.

❧                      ❧                      ❧

The sooner political propaganda against archaeological work by foreigners in Iraq is dropped, the sooner scholars will be able to feel some confidence in the measures at present being taken in that country. The present trend of events can only lead disinterested observers to believe that irreparable harm is being done by a policy which discourages and penalizes scientific expeditions without providing efficient means to prevent wholesale spoliation and destruction of ancient sites. The threat of various new laws has been disturbing. If a new law is introduced next year on the lines apparently proposed, the Iraq Government will have placed itself in the position of stopping work by others, while itself unable to prosecute research.

❧                      ❧                      ❧

With the publication of this number we welcome the accession of a group of new readers in the United States of America. We welcome them most heartily. Already for several decades American and British institutions and individuals have been engaged upon joint undertakings throughout the world, particularly in the East. Ur was excavated by just such a joint expedition, and several others are still in the field. Such cooperation is characteristic of all live science, but the effects are often limited, of necessity, to the few persons directly concerned.

❧                      ❧                      ❧

It has been one of the tasks of ANTIQUITY to interpret to a wider public the meaning of the epoch-making discoveries so made. For without the cooperation of the public, archaeology as it exists today could not continue—a fact liable to be forgotten by specialists when in full cry! It is obviously therefore in the interests of all concerned that some one should broadcast the chief items of news in language as



## EDITORIAL NOTES

plain as the subject permits. We have attempted to do this in the past and we shall continue to do so, encouraged by the knowledge that our efforts are appreciated in at least two Continents. There will of course be no change of policy where none is needed ; we shall merely try and do better what we have hitherto been doing, and perhaps cast our nets even more widely than before, particularly in eastern waters.



Amongst undertakings which (so far as staff is concerned) may well be called international, are the various expeditions of the Oriental Institute of Chicago University, which is doing magnificent work throughout the whole of the Middle East, under the direction and personal inspiration of Dr J. H. Breasted.\* The major results obtained are already fairly well known, and as publication proceeds they will become even better known. Amongst the recent publications of the Institute are some (vols. x, 1929 ; xvii, 1933 ; xviii, 1934) which deal with the late Tertiary and Quaternary Deposits of Egypt, with special reference to the antiquity of man. Dr K. S. Sandford has been studying these deposits since 1926, when Dr Breasted provided the organization required to ensure that a comprehensive survey, extending over a period of years, should be carried out. Details of the work done between Cairo and the Second Cataract are now available, and considerable progress has been made between Dongola and Berber. Palaeolithic implements have been found in geologically dated strata, and there is much evidence of progressive desiccation. Further volumes in the series will be awaited with interest.



We conclude with a word to our 'constant readers'. We are engaging in a publicity campaign to secure fresh subscribers, for even after eight years of effort there must remain quite a few people who are not yet aware of the existence of *ANTIQUITY*. We are therefore posting to several thousand members of various Societies in this country and abroad leaflets similar to those by which *ANTIQUITY* was established.

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\*The value of the work of The Oriental Institute generally is also emphasized by the Review on page 109.

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It is quite certain that some of those who already subscribe will receive leaflets, for the process of elimination of names to avoid this is impossible, and indeed the labour would be out of all proportion to the results. We therefore ask them, if they receive leaflets, to send them to friends who may be interested and thereby help the campaign, or to ignore them.



Again, at the risk of being thought importunate, we ask the attention of those who may be concerned to the notice printed below.

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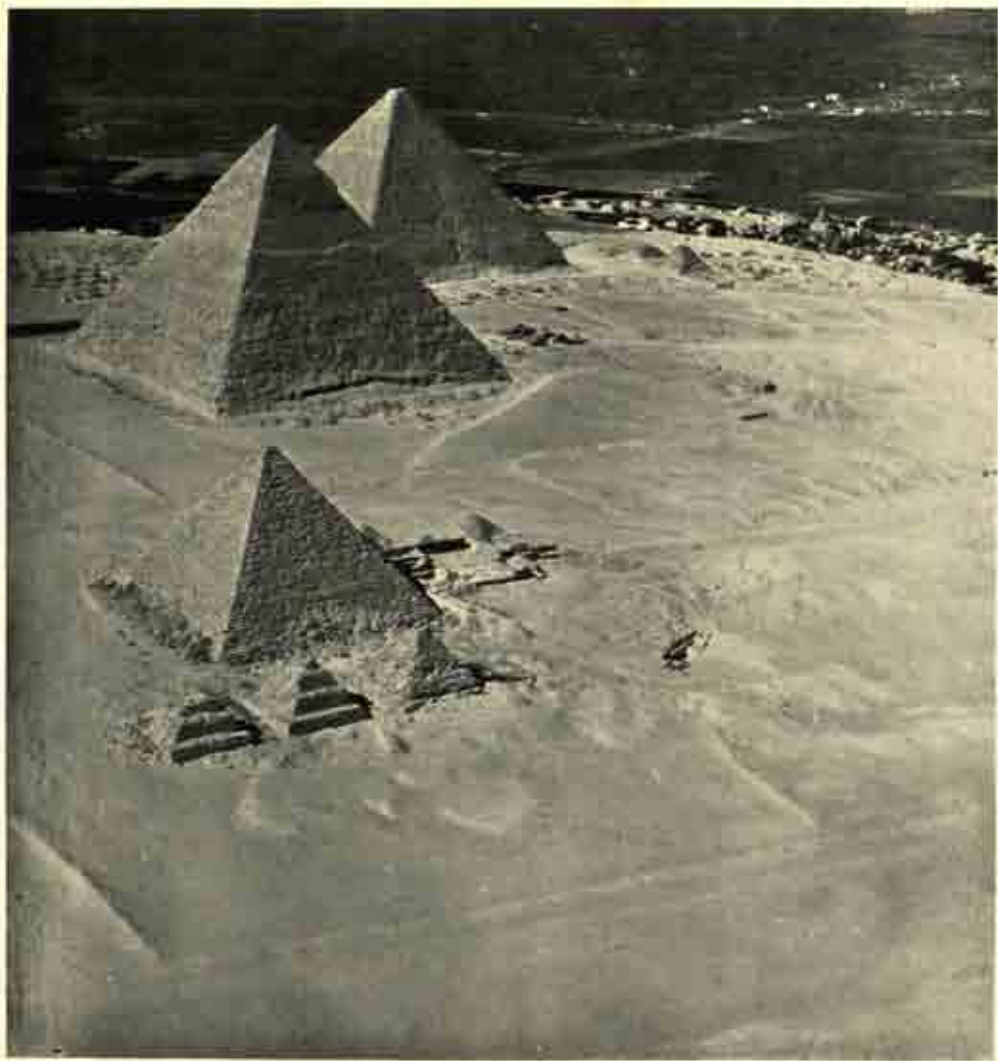
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PLATE II



THE PYRAMIDS AT GIZA, FROM THE AIR (See p. 8)

## Pyramids and their Purpose

by NOEL F. WHEELER

THE main object of these articles is to examine the evidence on the subject of Pyramids in general and the Great Pyramid of Khufu (Cheops) in particular; and to see if a reasonable conclusion can be reached as to the purpose for which the Great Pyramid was erected.

It is necessary to do this because the general reader is often at the mercy of what he reads, and unable without some guidance to distinguish between journalistic rubbish and scientific statements. He is apt to judge the importance of a document rather by the forcefulness of its dogmatism, the extent of its publicity, or the 'interest' of its theories, than by the scientific value of its content; and while there are some publications dealing with the Great Pyramid which are worth reading, there are many which are not, though they attract much more attention. After reading these articles the reader will be in a position to decide for himself whether he should accept without question the wild and fantastic theories current, or whether he should keep to ordinary commonsense and archaeological evidence.

In order to provide a background to the subject, I propose to discuss first the Pyramid Age and to consider for a moment what sort of people were the Egyptians of that time. After that (in a second article) I shall describe the Great Pyramid in some detail, in the light of recent excavations around it, and try to arrive at a commonsense solution of its many problems. Finally (in my third article) I shall touch briefly on some of the wilder theories, but shall treat them no more seriously than they deserve.

### THE PYRAMID AGE

What is usually described as the Pyramid Age is the period of the third, fourth, fifth and sixth dynasties, covering approximately 500 years in time from 3000 to 2500 B.C. The actual dates attributed by



## ANTIQUITY

different writers to the kings of this period vary so much that their only significance in the present connexion is relative. The initial date of 3000 B.C. may well be as much as 100 years out in either direction; and it is not until one comes to the next period, the Middle Kingdom, about 2000 B.C., that dates can be fixed with any degree of certainty.

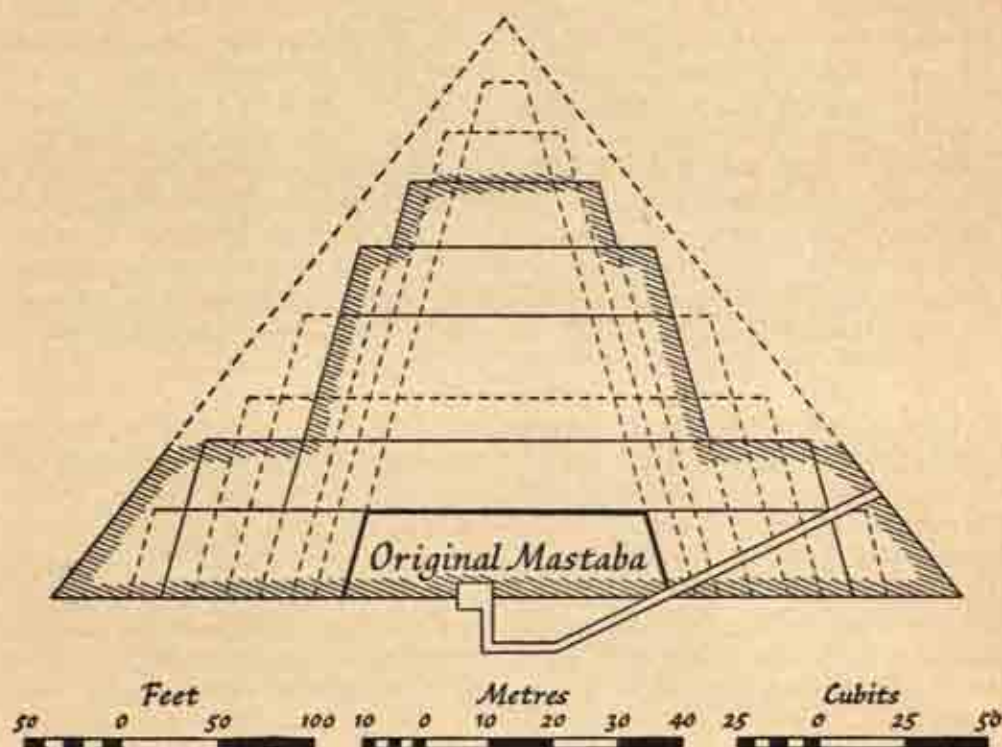
The third dynasty, which began and ended around 3000–2900 B.C., included the well-known king Zoser (variously written Thoser, Zeser, etc.), who was the builder of the step-pyramid at Sakkara (where the Egyptian Government has excavated during recent years some of the most interesting architectural works ever found in Egypt); and king Huni (Nefer-ka-Re Huni), the presumed father of the great king Seneferu who began the fourth dynasty. Seneferu may be met with in various writings as Snofru, Snefru, Seneferuw, Soris (Manetho), and other versions even further from his true name which is in the hieroglyphic 'S-nfr-w'. During the fourth dynasty, which begins with Seneferu, there were the Pyramid kings of Giza—Khufu (Cheops, Sufis, Khnum-Khufu, etc.); Re-dedef (Rhatioses, Dedefre, etc.); Khafre (Sufis, Chephren, etc.); Menkauré (Menkheres, Mykerinos, etc.); and Shepses-kaf (Bikheris, etc.).

The fifth dynasty includes the Pyramid kings of Abusir—User-kaf (Userkheres, Weser-kaf, etc.); Sahuré (Sefres); Nefer-ir-ka-ré (Neferkheres, etc.); Nefer-ef-ré (Kheres, etc.); Ne-user-ré (Rhatihures, Ra-en-user, etc.); Isesy (Ded-ka-re Assa, Tankheres, Isosy, etc.); and Unas (Wenis, Onnos, Unnos, etc.). Among the kings of the Sakkara Pyramids (sixth dynasty) were Tety (Teta, Othoes, etc.); User-ka-re; Mery-Re Pepy (Fiops, Pepy I); Merenre (Methusufis, etc.); Nefer-ka-re Pepy (Pepy II, Fiops, etc.); Merenre II.

The type of royal tomb in vogue before the introduction of the pyramid was the mastaba—a rectangular structure of masonry whose length was usually a little more than twice the breadth, flat-topped, and with the outer faces rising at a slope of about 7 to 14 on one. (The actual angles of slope at Giza vary from  $74^{\circ} 4'$  to  $80^{\circ} 57'$ ). The mastaba tomb continued in use alongside the later pyramid throughout the Pyramid Age, and varied in size from the very small mud-brick mastabas of the poorer people, a few feet square, to the large royal mastabas at Giza, of which the largest has a length nearly one half of the side of the Great Pyramid. (See FIGURE opposite).

During the third dynasty the pyramid idea began to develop out of a series of superimposed 'mastabas'; and the step-pyramid of Zoser at Sakkara, the step-pyramid at Meydum, a pyramid at Zawiet-el-Aryan

## PYRAMIDS AND THEIR PURPOSE



SECTION OF THE PYRAMID OF MEJDUM, SHOWING DEVELOPMENT FROM ORIGINAL MASTABA  
PRESENT OUTLINE SHADED  
(Petrie, 'History of Egypt' vol. 1.)



## ANTIQUITY

possibly of king Kha-ba and possibly a step-pyramid (between Giza and Abusir), and an unfinished pyramid at the same place possibly of king Neb-ka, all belong to this transitional period. Together with the pyramids of Seneferu and Huni (?) at Dahshur (south of Sakkara) and at Meydum (further south), none of these were true pyramids, but merely structures of a generally pyramidal form consisting of superimposed mastabas, which gave the appearance of steps. The slope of the faces of these steps was approximately that of the normal mastaba, and the whole was cased in fine limestone, still keeping the step form.

The first true pyramid, designed as such from the beginning, was that built by Khufu at Giza, known as the Great Pyramid (PLATE II). The other pyramids at Giza, and all subsequent pyramids not only at Giza, but at Sakkara and Abusir of the fifth and sixth dynasties, and of later periods at Dahshur, Lahun, Lisht, Hawara, Gurnah, Abydos and in the Sudan, were all true pyramids in the same sense and presumably directly descended from the Great Pyramid. The largest of all these, and the finest in workmanship, was the Great Pyramid, with a base side of 440 Egyptian cubits. With the exception of the Second Pyramid at Giza (of Khafre), and of two of the Dahshur pyramids, all the others are of much smaller dimensions. Confining ourselves to the true Pyramid Age the smallest recorded is that of Sahure's Queen at Abusir, which has a base side of only 30 cubits. The value of this cubit, and other matters concerning units of measurement will be considered in more detail later. (1 cubit = 20.59 inches or 52.3 cms.).

The publications on the subject of the Great Pyramid have been so numerous and so widely read that the non-archaeological reader might well forget that, besides the other Giza pyramids, there are a vast number of pyramids in Egypt alone—not to mention those in the Sudan. In order to give a clearer idea of them as a whole a list is given in the appendix (pp. 15-21), numbered consecutively, and it is by these numbers that they are referred to in the text.

It will be seen from the list that the first pyramids were built at Sakkara and Meydum, with two exceptions at Zawiet-el-Aryan—a few miles south of Giza. The change of site to Giza was made by Khufu, and he was followed there by the remainder of his dynasty, except Dedefre, who for some unknown reason went ten miles further north to Abu Roash for his pyramid. The fifth dynasty again shifted to Sakkara and Abusir, a few miles north of Sakkara; the eleventh dynasty moved up to Gurnah in Upper Egypt; the twelfth returned

PLATE III



AN UNDISTURBED BURIAL OF THE PYRAMID AGE. (See p. 9)  
The stone-slab lid of the rock-cut sarcophagus, the oblong jars, and the alabaster jars behind, are still in place



PLATE IV



THE ALABASTER SARCOPHAGUS OF HATHORHES, MOTHER OF KHUTU (CHUUPS) CAVED IN WOOD COMING OUT OF HER TOMB. (See p. 12)  
Dr. Reiser is standing at the right hand of the derrick.

PLATE V



SOLID GOLD HIEROGLYPHS SET IN EBONY STRIPS ON THE BACK OF THE CARRYING-CHAIR OF HETEP-HERES, MOTHER OF KHUFU (CHEOPS), CAIRO MUSEUM. (See p. 42)





## PLATE VI

ONE OF THE SLATE  
TRIADS SHOWING  
MENKAURE WITH  
HATHOR AND THE  
GODDESS OF THE  
JACKAL - NOMES,  
CAIRO MUSEUM.

(See p. 13.)

*Service des Antiquités,  
Cairo*

## PYRAMIDS AND THEIR PURPOSE

north to Lisht, Dahshur, Lahun and Hawara—none of them very far south of Sakkara, the farthest being about forty miles.

In addition to Egyptian pyramids, there were Roman pyramids, the Pyramid of Cestius at Rome being 116 feet high; the pyramidal mounds of early Chinese emperors; and many 'freak' pyramids all over the world. Copernicus built one over a well at his house in Frauenburg near the mouth of the Vistula. There is one, of very small dimensions, in the Dean cemetery, Edinburgh. The pyramidal temples in America, which have been often described as pyramids, belong to quite a different family from the Egyptian Pyramids: they are stepped, have a very shallow slope, and were surmounted by a temple—showing that their purpose was entirely different. The Egyptian pyramid was essentially the superstructure of a tomb, which was nearly always cut in the rock beneath, the entrance being in one face of the pyramid or in the rock outside it (PLATE III).

There may have been other pyramids at Abusir, Abu Roash, and at Gurnah, all trace of which has now gone. Either they are buried under débris in unexcavated areas, or they have been razed to the ground by plunderers and the elements.

It is possible to get very close to an understanding of the people of the Pyramid Age; but to do so one must see such of their handiwork as remains to us, scattered through the museums of the world, and study the literature of the subject. In order to familiarize oneself with their handiwork there is no better method than to spend a long period on the site of their activities.

Egyptian craftsmanship had been developing rapidly from the time of the first dynasty, with a leap to stone building at Sakkara in the third; and it reached its zenith in the fourth and fifth; in the sixth dynasty the first signs of retrogression appear. Of Egyptian art at its zenith the late Mr Arthur Weigall says '[it] had now reached a level which, in certain respects, can hardly be said ever to have been excelled in that country. . . .' (*Ancient Egypt*, Benn, 1928). Petrie, in *Revolutions of Civilisation* (Harper, 1922), describes the art of Seneferu as 'almost perfect'. Referring to architecture, sculpture and painting, Terence Gray writes in his chapter on Khafré in Mrs Brunton's *Kings and Queens of Ancient Egypt* (Hodder and Stoughton, 1924), 'Under Khafré these things, evolving uncertainly during the preceding centuries, burst suddenly into a glorious first blossoming whose virile simplicity and primitive untrammelled inspiration have



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left them from the lifetime of Khafré until today among mankind's most masterly artistic achievements'. Concerning their literature we read in Professor Blackman's translation of the *Literature of the Ancient Egyptians of Erman* (Methuen, 1927), 'As far back as we can trace it, the Egyptian language displays signs of being carefully fostered. It is rich in metaphors and figures of speech, a "cultured language" which "composes and thinks" for the person who writes. One at least of the old books of proverbs may even have been composed during the Old Kingdom, in the time of the fifth dynasty (c. 2700 B.C. or earlier), which is known to us as an age in which the plastic art was at a particularly high level'.

There is no doubt that the Egyptians of the Pyramid Age were a very remarkable people, and that they had developed exceptional technical and artistic skill; but it is not possible to realize just how remarkable were their achievements without acquiring some first-hand familiarity with them. They possessed many qualities well developed which we today do not associate with 'the Orient'—which in fact belong nowadays solely to some of the northern races; and, certain though it be that the ancient Egyptians possessed these qualities, it is equally certain that the same qualities are entirely absent in the East today. They were clear and logical thinkers, systematic in all they did; they were persevering and remarkably accurate in executing plans given them, being in no way satisfied with 'near enough'—it had to be as exact as their instruments and knowledge would allow; they were in no way afraid of tackling the most difficult mediums in which to work, and preferred them at this time (the Pyramid Age) to the more easily worked materials. They had a simple, refined and restrained taste in art, being not at all fond of the elaborate over-decoration which characterized later work, and their eye for correct detail was wonderful.

Those who are familiar with the art of the Pyramid Age consider that it reached a level unsurpassed by the art of the Middle Kingdom, the New Kingdom, or that of Saite and Ptolemaic times. The interesting objects from the eighteenth dynasty tomb of king Tut-ankh-amen, though amongst them are some beautiful things, do not reveal the same taste and refinement as the products of the Pyramid Age.

Undoubtedly a people who were capable of such masterpieces as we can still see must have had very orderly minds, and much the same ideals of art and conduct as the most advanced of modern peoples. They had a deep knowledge of nature in all her aspects, and their



## PYRAMIDS AND THEIR PURPOSE

representations of country scenes, animals, birds, and fish, would lead us to expect that their feelings for animals more nearly approximated to our own than to those of their modern descendants. Another very surprising aspect of their art, in view of its antiquity and expected primitiveness, is its cleanness; there are no signs of immoral or perverted tendencies, and the only representations seen which we are not accustomed to see in public today had an obviously religious, magical, or natural significance. We can be sure that much to which we are accustomed today would have shocked them as entirely improper.

In the matter of their religion we are apt to be misled by the prejudiced views of the Hebrews in their writings. There is no doubt that the Egyptians and the Israelites did not love one another; but whereas the Egyptians did not consider the Israelites of sufficient importance to warrant more than casual mention, the Israelites attributed every conceivable unpleasant quality to the Egyptians. At the time of the pyramids the Egyptian religion was not so complicated and inconsistent as it became later, mainly through the effort to combine a number of different beliefs.

It will be desirable here to mention some of the outstanding monuments of the period.

Of the third dynasty the most striking find is the complex of temples around the step-pyramid of Sakkara (King Zoser). This was the first big architectural adventure which the Egyptians made; they translated into stone the age-long building methods of mud-brick, timber and reeds. The step-pyramid area was being excavated by the Egyptian Government under the direction of the late Mr Cecil Firth when the latter died about two years ago. Since then the work has halted. The ordinary visitor, with no great knowledge of Egyptian work, finds it hard to believe that these buildings are not Greek! The simplicity, grace and technical skill is astounding.<sup>1</sup>

Of the time of Seneferu (fourth dynasty), there are in the Cairo Museum two very fine painted limestone statues, life-size, of Prince Rahotep and Princess Nefert (PLATE I, *frontispiece*). These two statues are remarkably life-like and the colouring is in very good condition; the statue of Nefert in particular shows extreme freedom in handling. They were found by Daninos, in the time of Mariette, in one of the

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<sup>1</sup> See *ANTIQUITY*, 1928, II, 461-3.



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mastabas near the Meydum Pyramid, which itself shows a standard of skill in building scarcely exceeded later.

One of the finest pieces of work of this date (Seneferu) is the painting on plaster of six geese, removed many years ago by Vassalli Bey from one of the Meydum mastabas, and now in the Cairo Museum. The geese are feeding, and the detail of drawing and colour are very good. Also in the Cairo Museum are three wooden panels from the 'false-door' of Hesi-Ra at Sakkara, carved with the figures of Hesi and his name and titles in hieroglyphics in slightly raised relief. The style is extremely delicate and fine, with perfect detail. The panels have often been published.

In the next reign, that of Khufu, the best known work is the Great Pyramid of Giza, whose boldness of conception and meticulous care in execution cause it to stand out from all other monuments of Egypt. To the same date belong inscribed and uninscribed sarcophagi of granite, alabaster and limestone, in the Cairo, Boston and British Museums. They vary in style, from that of Khufu-nekht in Cairo, and others like it, with intricate panelling design, to the absolutely plain and undecorated alabaster sarcophagus of Khufu's mother (Cairo). There is an ivory figure of Khufu in the British Museum which is interesting mainly because we are very badly off for representations of this king.

By far the greatest find of this reign, however, was the intact secret tomb of Khufu's mother Hetep-heres at Giza.\* This tomb was found and excavated by the Harvard-Boston Expedition during 1925-1926; and the present writer shared the work of excavation and record with Dr G. A. Reisner, the Director of the Expedition. No intact royal tomb of this age had been found before, and the richness of the furniture and other objects, together with the perfect taste of the designs and decoration make this collection in the Cairo Museum the most remarkable to be found there. The chairs, bed, canopy and jewel-box were not covered with thin gold-leaf, as much of the later work was, but with heavy gold sheeting; the gold was decorated with a mat pattern in relief, and the hieroglyphics on the pillars of the canopy are in raised relief on the gold, executed with almost perfect detail. On the 'carrying chair' solid gold hieroglyphics were set in ebony strips. There is not a scrap of superfluous decoration, everything is in perfect taste, and the workmanship is first-class. (PLATES IV-V).

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\* See ANTIQUITY, 1927, I, 216-8.



## PYRAMIDS AND THEIR PURPOSE

Of the reign of Khafré, who built the second Pyramid of Giza, the finest remaining work is the diorite statue of the king in the Cairo Museum. It was discovered by Mariette in the well of the granite valley-temple along with the debris of eight other statues of Khafré (also in Cairo). It is a wonderfully executed piece of work in one of the hardest and most difficult of stones. All the detail is rendered with marvellous vigour and truth to nature, and the whole figure expresses dignity, repose and power in a remarkable degree. Petrie writes in his *History of Egypt*: 'The statues of Khafra have brought us face to face with him, and caused his features to be almost as well known in our times as in his own reign. The great diorite statue is a marvel of art, the precision of the expression combining what a man should be to win our feelings, and what a king should be to command our regard. The subtlety shown in this combination of expression—the ingenuity in the over-shadowing hawk, which does not interfere with the front view, the technical ability in executing this in so resistant a material—all unite in fixing our regard on this as one of the leading examples of ancient art'. There is also, among the other statues of this king in Cairo, a very fine green basalt figure, found in the same place as the diorite statue, and an alabaster statue which is of fine work and was found at Sakkara.

In the reign of Menkauré, the king of the third Giza Pyramid, fragments of whose bones and sarcophagus are in the British Museum, by far the finest objects are the portrait statues and the triads in slate, found by the Harvard-Boston Expedition in excavating the valley temple of the king's pyramid (PLATE VI). There are no more beautiful objects from Egypt than these slate triads of the king, Hathor, and different nome-deities; and the slate pair-statue of Menkauré and his queen Kha-merer-nebty II. Slate is not a material to choose for ease of working, yet the execution of these statues is almost perfect and they have to be seen to be believed. Of the triads three are in Cairo and the remainder in the Boston Museum; the slate pair is in Boston; and there is a fine alabaster statue in Cairo. These have been illustrated in several publications, but should be looked for in Reisner's *Mycerinus* (Harvard University Press, Cambridge, Mass., 1931). The basalt sarcophagus of Menkauré was very fine, decorated with panel design, but unfortunately when Howard Vyse had removed it nearly a century ago it was lost at sea on its way to the British Museum.

Coming next to the fifth dynasty; in the reign of Nefer-ir-ka-ré we have the tomb of the great noble Ty, the walls of which are covered



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with very fine coloured relief scenes. In the Cairo Museum is the standing statue of Ranofer in painted limestone, one of the best examples of the work of this period.

The 'Precepts of Ptah-hetep' were probably written in the reign of Dedkaré-Issy, though the earliest surviving papyrus is of the Middle Kingdom. Ptah-hetep may have been an aged tutor of the king. The 'Prisse Papyrus' in the Bibliothèque Nationale was bought from one of the fellahin by M. Prisse d'Avennes and published in 1847; it is over 23 feet long and about 6 inches wide. Also from this reign is the Palermo Stone, a fragment of a list of kings from the first to the fifth dynasty, broken out of the centre of a large slab of diorite. This has been in Palermo since 1877.

In the reign of Unas there was a change in customs. The Sun-Temples, built by the preceding kings, came to an end; and the Pyramid Texts began. These latter were religious texts inscribed on the walls inside the pyramids and are exceedingly interesting though difficult to interpret.

There are a number of objects from the fifth dynasty which cannot be placed definitely in any one reign. Of these the best known and finest are two statues, the 'Sheikh-el-Beled' and the 'Seated Scribe'. The 'Sheikh-el-Beled' (a name invented since its discovery) is a wooden statue found by Mariette at Sakkara, and now in Cairo. It represents a noble called Ka-aper. This statue ranks with the diorite Khafré and is a very fine piece of work; Maspero in his *Histoire générale de l'Art : Egypte* (Hachette, 1912), says: 'Le Cheikh-el-Beled marque l'apogée de l'art memphite, et, si l'on ouvrait quelque part un salon des chefs-d'œuvre du monde entier, il est un de ceux que j'y enverrais pour l'honneur de l'art égyptien'. The second of these two statues, the 'Seated Scribe', is in the Louvre and ranks in the same class: the expression and attitude are both perfect. There are a number of other statues of seated scribes in the Cairo Museum, but not one can be compared with that in the Louvre.

Of the time of Teti, sixth dynasty, there is a fine bronze statue of the king in the Cairo Museum which was found at Hierakonpolis. The bronze was originally on wood, but the wood has completely decayed and the statue was found in fragments of bronze sheet and has been reconstructed.

Innumerable monuments of these dynasties are not mentioned, since I have only intended to give some idea of the artistic achievements of the Pyramid Age.

# PYRAMIDS AND THEIR PURPOSE

## APPENDIX

### LIST OF PYRAMIDS IN EGYPT AND THE SUDAN

The dates are only relative and approximate, and the dimensions are given in Egyptian cubits, hands, and fingers. (The cubit contains seven hands, and the hand four fingers). The angle of slope of the pyramid-face is given in the manner of the Egyptian method of stating a slope, namely of a vertical rise of one cubit on a horizontal base of so many hands and fingers. For example, a slope of 5-2 signifies a rise of one cubit on a base of five hands and two fingers, or an angle as we express it today of  $51^{\circ} 51'$ . Under the 'base' and 'height' columns the measurement is given as 92.2.1, meaning 92 cubits, 2 hands, and 1 finger; sometimes, when the fraction is exact, the measurement is given as  $195\frac{1}{2}$ , etc., meaning  $195\frac{1}{2}$  cubits.

No.	Date B.C.	Place	Description and Owner	Dynasty	Base c. h. f.	Height c. h. f.	Slope h. f. deg. min.
1	2840?	SAKKARA	Step-pyramid Zoser	III	205.0.3 and 225.5.3	112.0.0	2.1 72.30
2		MEYDUM	"	III	274.0.0		2.0 73.30
3		ZAWIET- EL-ARYAN	"	III	158.0.0	$195\frac{1}{2}$	2.3 68
4		"	Kha-ba?	III			
5	2824	DAHSHUR	Unfinished. Neb-ka?	III			
6		"	North Stone. Huni or Seneferu	III-IV	408.0.0		7.1 43.40
7	2824	MEYDUM	South Stone. 'Blunted Pyramid'		360.0.0	168.0.3	7.2 43. 1 $\frac{1}{2}$
8	2800	GIZA	'False pyramid' 'El Haram el Kaddab'	IV	275.0.0	175.0.0	5.2 51.51
9	2800	"	GREAT PYRAMID, Khufu	IV	440.0.0	280.0.0	5.2 51.51
10	2800	"	A queen of Khufu	IV	92.6.0		
11	2800	"	A queen of Khufu	IV	92.2.1		
12	2770	ABU ROASH	Queen Henutsen	IV	88.1.2	56.5.0	5.1 $\frac{3}{4}$ 52.10
13	2760	GIZA	Dedefré	IV			
14	2760	"	2nd pyramid. Khafré	IV	410.0.0	272.0.0	5.1 53. 8
15	2750	"	A queen of Khafré	IV	40.0.0	$26\frac{1}{2}$	5.1 53. 8
16	2750	"	3rd pyramid. Men- kauré	IV	200.0.0	124.2.0	5.3 51.10
17	2750	"	Menkauré's queen, Khamerernebty II	IV	98.2.0	60.4.3	6.2 51
18	2750	"	A queen of Menkauré	IV			
19	2700?	"	A queen of Menkauré	IV			
20	2680	SAKKARA	Queen Khent-kau-es 'HaramelMaharbish' Userkaf	IV V			



# ANTIQUITY

## APPENDIX—cont.

No.	Date B.C.	Place	Description and Owner	Dynasty	Base c. h. f.	Height c. h. f.	Slope h. f. deg. min.
21	2670	ABUSIR	North pyramid. Sahuré	V	150.0.0	89.2.3	5.3 50.36
22		"	Sahuré's queen	V	30.0.0	21.3.0	4.3 55.50
23	2660	"	Nefer-ir-ka-ré	V	200.0.0	133.3.0	5.1 53.8
24		"	Khent-kau-es, queen of Nefer-ir-ka-ré	V			
25		"	Unfinished. Nefer- ef-ré	V			
26	2640	"	Ne-user-ré. Called pyramid of Riga, or Abu Gurob	V	150.0.0	95.3.2	5.2 51.51
27	2660	SAKKARA	Unas	V	128?		
28	2640	ABUSIR	Ne-user-ré's queen	V			
29	2570	SAKKARA	Teti	VI	122.3.0		
30	2570	"	Teti's queen?	VI			
31		"	Queen Yput, mother of Pepi I	VI			
32	2510	"	Pepi I	VI	146?		
33		"	Merenré	VI			
34		"	' Haram el Shawaf '	?			
35		"	Pepi II	VI			
36		"	Queen Wezebtén, wife of Pepi II	VI	31.4.0		
37		"	Queen Yput	VI			
38	2625	"	Neith, queen of Pepi II	VI			
39		"	Kakaré Yby	VII			
40	2500?	GURNAH	Yntef I	XI			
41		"	Mentuhotep III	XI			
42	2040?	LISHT	North pyramid. Amenemhat I	XII			
43	2000	"	South pyramid. Senwesert I	XII			6.1 48.53
44		"	Small pyramid	XII?			
45		"	"	XII?			
46		"	"	XII?			
47	2000	"	Queen of Senwesert I	XII			
48	1938	DAHSHUR	' White pyramid '. Amenemhat II	XII			
49	1906	LAHUN	Senwesert II	XII			9.1 42.35
50	1887	DAHSHUR	North Brick pyramid Senwesert III	XII	204?		

# PYRAMIDS AND THEIR PURPOSE

## APPENDIX—cont.

No.	Date B.C.	Place	Description and Owner	Dynasty	Base c. h. f.	Height c. h. f.	Slope h. f. deg. min.
51	1849	DAHSHUR	South Brick, or 'Black Pyramid.'				
			Amenemhat III	XII	200?		
52	1849	HAWARA	Amenemhat III?	XII			6. $\frac{1}{2}$ 48.45
53		DAHSHUR	A queen's pyramid	?			5.3 $\frac{1}{2}$ 50
54		SAKKARA	Userkaré Khénzer	XII			
55	1850?	GURNAH	Nub-kheperé Yntef	XVII			
56		"	Sekhemré Upmaat				
		"	Yntef	XVII			4.0 60
57		"	Sekhem-Shedtauy-				
		"	Sebekemsaf	XVII			
58		"	Sekenenré Ta-a	XVII			
59		"	Sekenenré Ta-a-a	XVII			
60	1700?	"	Waz-Kheperre Kames	XVII	15.2.0	17.1.1	3. $\frac{1}{2}$ 66
61	1700?	ABYDOS	Ahmes I	XVIII			
62	1640	"	Queen Tetisheri	XVIII			
63	750	KURRU	No. 8. Kashta	XXV			
64	744	"	No. 17. Piankhy	XXV			
65	710	"	No. 15. Shabaka	XXV			
66	700	"	No. 18. Shabataka	XXV			
67	688	NURI	No. 1. Taharka	XXV	98.6.0	128.5.0	2.2 $\frac{1}{2}$ 69
68	688?	"	No. 21. A queen	XXV			
69	688	"	No. 35. A queen?	XXV			
70	663	KURRU	No. 16. Tanutaman	XXV			68?
71	653	NURI	No. 20. Atlanersa	XXV	23.0.3	26.3.0	66 $\frac{1}{2}$
72	653	"	No. 36. Queen				
		"	Atakhibasken	XXV			
73	643	"	No. 3. Senkamanseken	XXV	52.5.1	59.1.2	66
74	640?	"	No. 41. Queen				
		"	Mara-?n	XXV			
75	?	"	No. 42. ?	XXV			
76	?	"	No. 43. ?	XXV			
77	623	"	No. 6. Anlaman	XXV	53.6.2	46.4.3	60
78	623	"	No. 22. Queen of				
		"	Anlaman	XXV			
79	623	"	No. 23. Queen				
		"	Masleya	XXV			
80	593	"	No. 8. Aspalta	XXV	53.1.1	49.6.3	62
81	593	"	No. 24. Queen				
		"	Nansalaa	XXV			
82	593	"	No. 27. Queen				
		"	Madigan	XXV			

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## APPENDIX—*cont.*

No.	Date B.C.	Place	Description and Owner	Dynasty	Base c. h. f.	Height c. h. f.	Slope h. f. deg. min.
83	593	Nuri	No. 38. Queen Aaqata	XXV			
84	?	"	No. 39. ?	XXV			
85	?	"	No. 40. ?	XXV			
86	563	"	No. 9. Amtalqa	XXV	53.0.1	65.4.0	68?
87	563	"	No. 26. Queen Amantakaya	XXV			
88	553	"	No. 5. Malenagan	XXV	53.1.0	65.5.0	68?
89	553	"	No. 25. Princess Maletalen?	XXV			
90	553	"	No. 28. Queen Henut-Takhabit?	XXV			
91	538	"	No. 18. Nalma'aya	XXV	20.3.0	25.2.3	68?
92	533	"	No. 10. Netaklabataman	XXV	54.0.0	46.5.2	60
93	513	"	No. 7. Karkaman	XXV	53.0.0	45.6.1	60
94	513	"	No. 30. Queen?	XXV			
95	503	"	No. 2. Astabarqaman	XXV	53½ & 51		59
96	500?	"	No. 29. Queen Piankh-har?	XXV			
97	473	"	No. 4. Sa'asheriqa	XXV	51.4.0	63.5.1	68?
98	458	"	No. 19. Nasakhma	XXV	18.4.1	22.6.1	68?
99	453	"	No. 11. Malewiya- man	XXV	51.2.0	52.4.0	64
100	453	"	No. 31. Queen?	XXV			
101	453	"	No. 32. Queen?	XXV			
102	423	"	No. 16. Talakhman	XXV	23.1.0	28.4.2	68?
103	418	"	No. 12. Amanheri- nutarik	XXV	51.0.2 & 62.1.0		62
104	418	"	No. 33. Queen?	XXV			
105	418	"	No. 34. ?	XXV			
106	398	"	No. 17. Baskakeren	XXV	23.6.2	29.3.3	68?
107	350?	"	No. 37. Queen?	XXV			
108	350?	"	No. 48. Queen?	XXV			
109	?	"	No. 49. ?	XXV			
110	?	"	No. 50. ?	XXV			
111	?	"	No. 51. ?	XXV			
112	?	"	No. 52. ?	XXV			
113 to 131	?	"	{ Nineteen small pyramids of Royal Ladies, south of No. 1 }	XXV	23?		



# PYRAMIDS AND THEIR PURPOSE

## APPENDIX—cont.

No.	Date B.C.	Place	Description and Owner	Dynasty	Base c. h. f.	Height c. h. f.	Slope h. f. deg. min.
132	397	NURI	No. 13. Harsiotef	XXV	51.2.3	63.3.1	68?
133	397	"	No. 44. Queen Banhaly	XXV			
134	?	"	No. 45. ?	XXV			
135	?	"	No. 46. ?	XXV			
136	?	"	No. 47. ?	XXV			
137	362	KURRU	No. 1. Piankhalara	XXV			
138	342	NURI	No. 14. Akhratan	XXV	50.6.2	62.6.2	68?
139	328	"	No. 15. Nastasen	XXV	50.2.3	62.1.3	68?
140	308	BARKAL	No. 11. ?	Na-1			
141	300	BEGARA- WIYAH	No. S6. Arikakaman	KM			
142	280	"	No. S5. Yesruwaman	KM			
143	275	BARKAL	No. 14. ?	Na-1			
144	265	BEGARA- WIYAH	No. S10. Kaltaly	KM			
145	265	BARKAL	No. 15. ?	Na-1			
146	255	"	No. 18. ?	Na-1			
147	255	BEGARA- WIYAH	No. N4. Aman-?- tekha	KM			
148	245	BARKAL	No. 7. ?	Na-1			
149	242	BAGARA- WIYAH	No. N53. ?	KM			
150	225	"	No. N7. Ergamenes	KM			
151	200	"	No. N9. Azagraman	KM			
152	180	"	No. N8. Nahirqa?	KM			
153	160	"	No. N11. Nahirqa?	KM			
154	150	"	No. N12. Shanek- zekheté	KM			
155	125	"	No. N13. ?	KM			
156	100	BARKAL	No. 6. ?	Na-2			
157	100	BEGARA- WIYAH	No. N20. ?	KM			
158	85	BARKAL	No. 4. ?	Na-2			
159	80	BEGARA- WIYAH	No. N21. ?	KM			
160	65	BARKAL	No. 2. ?	Na-2			
161	60	BEGARA- WIYAH	No. N14. ?	KM			
162	45	"	No. N2. Amanta- bale?	KM			
163	45	BARKAL	No. 9. ?	Na-2			

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## APPENDIX—cont.

No.	Date B.C.	Place	Description and Owner	Dynasty	Base c. h. f.	Height c. h. f.	Slope h. f. deg. min.
164	45	BARKAL	No. 10. ?	Na-2			
165	45	BEGARA- WIYAH	No. N6. Aman- shakhéte	KM			
166	25	"	No. W5. Queen ?	KM			
167	15	"	No. N22. Netekaman	KM			
168		"	No. N1. Queen Amantère	KM			
169	15	"	No. N10. Sherakarér	KM			
170	20	"	No. 15. ?	KM			
171	40	"	No. N16. Akhyes- tème?	KM			
172	50	"	No. N17. Amanitenmemize	KM			
173	75	"	No. N18. Amankhanewel?	KM			
174	100	"	No. N40. ?	KM			
175	105	"	No. N34. Artanyeszeme	KM			
176	130	"	No. N28. Tameqerze-amani	KM			
177	150	"	No. N41. ?	KM			
178	160	"	No. N29. Takizemani	KM	16½	21.5.3	2.2½ 72
179	180	"	No. N30. ?	KM			
180	200	"	No. N19. Tarekenizel ?	KM			
181	200	"	No. N32. ?	KM			
182	225	"	No. N55. ?	KM			
183	225	"	No. N37. ?	KM			
184	235	"	No. N38. ?	KM			
185	250	"	No. N36. Maniterara	KM			
186	270	"	No. N35. ?	KM			
187	270	"	No. N51. ?	KM			
188	290	"	No. N24. ?	KM			
189	310	"	No. N27. ?	KM			
190	330	"	No. N26. ?	KM			
191	340	"	No. N25. ?	KM			
192		"	No. N5. Prince Arik-Kharer	KM			

## PYRAMIDS AND THEIR PURPOSE

### APPENDIX—*cont.*

The dynasties from pyramids no. 140 to 192 in the list are indicated by the following abbreviations :—

Na-1	..	..	1st Meroitic dynasty of Napata
Na-2	..	..	2nd Meroitic dynasty of Napata
KM	..	..	Kingdom of Meröe

Pyramids 1 to 39 properly belong to the ' Pyramid Age ' ; 40 to 54 to the ' Middle Kingdom ' ; 55 to 62 to the ' New Kingdom ' ; and the remainder to the Ethiopian dynasties ruling at Napata and Meröe in the Sudan.

The Middle Kingdom pyramids are poorer than anything of the earlier period and those of the New Kingdom are mostly very small indeed and of very poor workmanship. The Ethiopian pyramids are quite a new departure. They have a much steeper angle of slope than the earlier examples, are quite small, and bear little resemblance to the great works of the Pyramid Age.



## Stukeley, Avebury and the Druids

by STUART PIGGOTT

THERE have been few tendencies in the history of English culture with so profound a contemporary influence as the so-called Romantic Movement of the 18th and early 19th centuries, and still fewer with such a strangely assorted progeny. That toying with 'the Gothick', which produced such early *jeux d'esprit* as Walpole's Strawberry Hill or Beckford's Fonthill, led, on the one hand, to the Albert Memorial, and, on the other, to the sculpture of Eric Gill; in literature, while the Romantics founded an honourable poetic tradition extending from Collins through Wordsworth to Blunden, it is surely not fantastic to see in such works as Lewis' *Bravo of Venice* the genesis of the modern thriller. Most strange of all, one outcome of the Romantic Movement was a new branch of science. For prehistoric archaeology in England was not the product of the classical lore so eagerly absorbed from Italy in the 16th and 17th centuries, but originated in those eccentric gentlemen of the 18th century who perambulated the countryside studying at first hand the antiquities of their own forefathers.

Easily the greatest of these early antiquaries was William Stukeley, and few individuals have left posterity such a mass of potential biographical material. While his published works are relatively few, he zealously kept every scrap of his own manuscript writings, his drawings, the proofs of his published engravings; his correspondence with antiquaries of the day; numerous notebooks, and twenty volumes of his diary, as well as two autobiographical essays. The main bulk of these papers was preserved by his descendants, the St. Johns of Dinmore Court, Herefordshire, and, with some additional material from other sources, were utilized by W. C. Lukis in his edition of *The Family Memoirs of the Rev. William Stukeley, M.D.* prepared for the Surtees Society in 1882-7. In 1924, some of the Dinmore Court MSS. were presented to the Bodleian<sup>1</sup> (which already possessed, in the Gough

<sup>1</sup> *Bodleian Quarterly Record*, October 1924, no. 43, 149.

PLATE I



WILLIAM STUKELEY AND HIS WIFE FRANCES

Drawing by Stukeley in a *ms Genealogy* (c. 1730) in the possession of Alexander Kellner





## STUKELEY, AVEBURY AND THE DRUIDS

topographical collections, several important Stukeley mss.) ; others were purchased by that Library and by Mr Alexander Keiller, who has since acquired additional mss. from time to time.<sup>2</sup> With such a plethora of material it is obviously impossible, in the space of a single article, to do more than indicate the main outlines of Stukeley's life, and to deal in detail with a single, but extremely important aspect of his archaeological work—the apparent mixture, in his published account of Avebury, of sound field-work with so much fantastic theorizing that in popular estimation the second characteristic has swamped the first. Most archaeologists today would probably endorse Tom Hearne's opinion of Stukeley—'He is a very fanciful man, and the things he hath published are built upon fancy'—but those who have had occasion to check his field-observations know him as an accurate and careful observer. A study of his life and thought as reflected in his own writings shows that this paradox is capable of explanation.

William Stukeley was born in 1687,<sup>3</sup> at Holbeach in Lincolnshire, his father, John Stukeley, being a lawyer in partnership with his elder brother Adlard. Engaging glimpses of his boyhood days can be gathered from his memoranda—his learning to write at the age of seven 'of Mr Coleman who taught us in the Quire of the Church'; or later, being taught to dance 'among the other young Fry of the Town'; and playing the flute, an accomplishment of which evidence exists today in a volume of flute music which he copied out in 1714.<sup>4</sup> We see him listening behind a screen to the conversations between his father and Mr Belgrave—an ingenious Gent—on astronomy, and writing an essay to controvert their arguments; making maps of the country round Holbeach, or a puppet theatre in imitation of one he had seen. In 1700 he was apprenticed in his father's office, but his inclinations did not lie in the study and practice of the law, and in response to his entreaties he was sent to Cambridge to study medicine, where he was admitted a pensioner at Corpus Christi in November 1703.

At Cambridge, Stukeley found himself in a congenial atmosphere. He attended Vigani's lectures on chemistry, went botanizing in the

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<sup>2</sup> The writer owes a debt of gratitude to Mr Keiller not only for indicating in the first place the possibilities of the mss., and suggesting the research of which this paper is an outcome, but for placing them unreservedly at his disposal and giving every facility for their study.

<sup>3</sup> Where the source is not otherwise given, the details of Stukeley's life are derived from Lukis' published work referred to above.

<sup>4</sup> A.K. MSS.



## ANTIQUITY

country around to collect material for a new edition of Ray's *Catalogus Plantarum*, and 'began to conceive a passionate Love for Antiquitys'. He gives a description of his room at Corpus :

'which had a very strange appearance with my furniture in it, the wall was generally hung round with guts, stomachs, bladders, preparations of parts and drawings. I had sand furnaces, Calots, glasses, and all sorts of chymical implements. . . . Here I and my Associates often dind upon the same table as our dogs lay upon. I often prepar'd the pulvis fulminans and sometimes surprized the whole College with a sudden explosion. I cur'd a lad once of an ague with it by a fright'.

One suspects that he was not wholly popular with those who inhabited rooms adjacent to his.

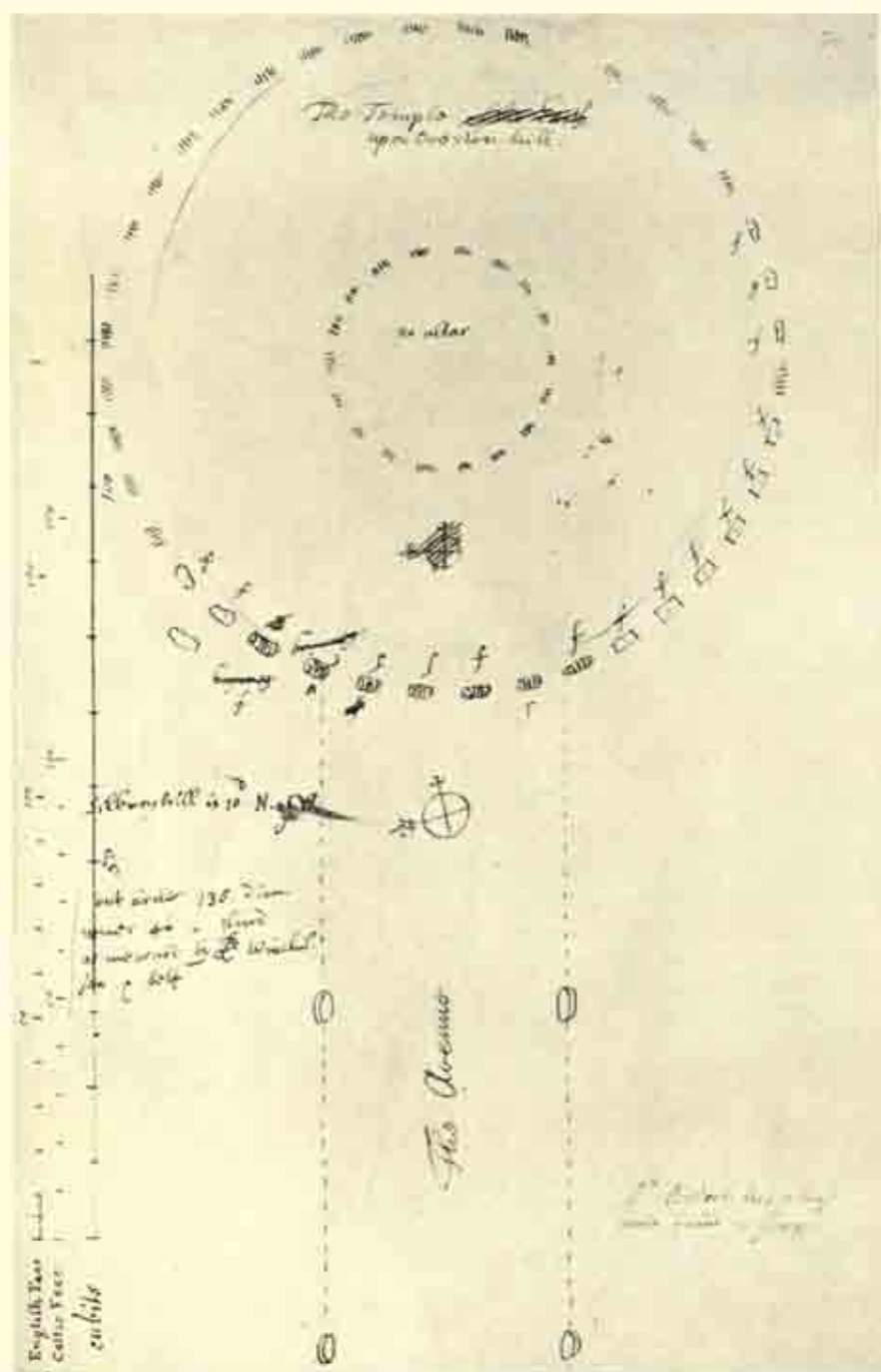
Stukeley's father died in 1705, his mother two years later, and on his taking his M.B. in 1709 he went to London to study at St. Thomas' hospital. Before this, however, he paid a visit to friends in Northamptonshire, where it appears that he was not insensible to the charms of his host's daughter Martha, who

'had somewhat of an airy temper, and accompanyd me in several of my Rambles in that Country to view Antiquitys, Roman Camps, and the like. We traveld together like Errant Vertuosos, and when we came to an old ruind Castle, etc., we climb'd together thro' every story and staircase, mutually helping one another, and pulling each other over the gaping arches and rugged heaps of rubbish, and when I had occasion to draw a view of them out, as we sat upon a stone or the grass, she held my ink horn or my paper, and was very serviceable and assistant in taking my designs, and all without any reserve or immodesty; nor could any aged Philosophers have convers'd together with more innocent familiarity or less guilt even in thought or intention. Nor could travailing curiosity or Antiquarian Researches be rendered so agreeable as with a fair and witty Companion and Fellow laborer, and when we return'd home my young Disciple could entertain the Family with so very curious Relation of the curiositys we had seen, that it would be difficult to say whether so nice taste in the Remains of Ancient Time most recommended a young Lady, or that Refined study became more lovely and delightful for her sake'.

There is a faint tang of bitterness and regret in that brief sentence which closes the episode in his 'Commentarys'—'She is since marry'd to a Gentleman in Wales'.

For the next ten years Stukeley practised medicine; in Lincolnshire at first, but from 1717 in London, where he was in 1720 elected a Fellow of the College of Physicians, reading the Gulstonian Lecture to that body in 1722. His wide range of scientific interests had already secured him a Fellowship of the Royal Society in 1717, but archaeology was rapidly becoming his principal pursuit. His interest in the antiquities of his own country was symptomatic of the feeling among the intelligentsia of his day, for already for some time 'a number of

# PLATE II

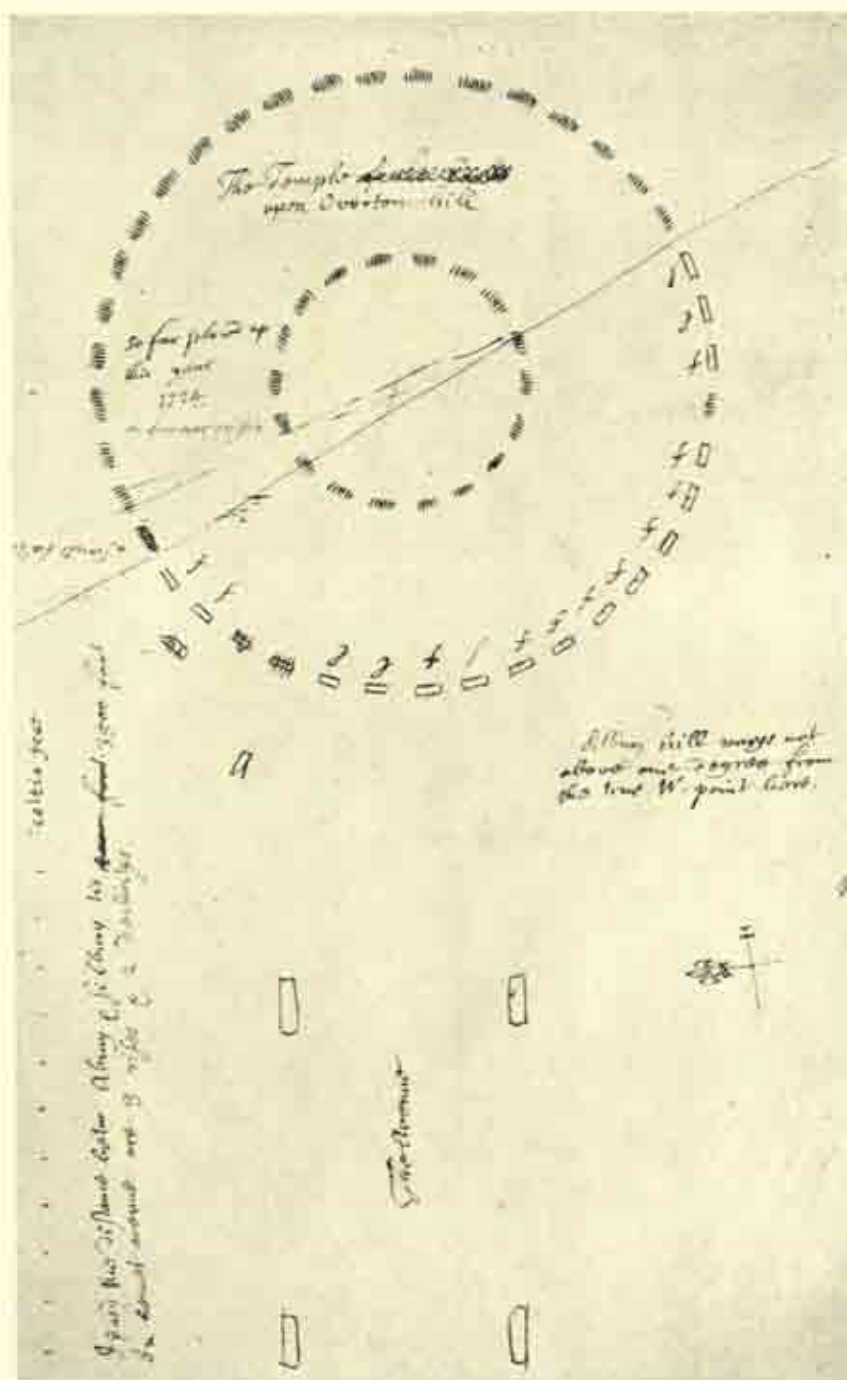


STURGELEY'S ORIGINAL FIELD-SKETCH OF THE PLAN OF THE OVERTON HILL STONE CIRCLES. [1724]

British Library, MS. Gough Maps 231.1.546



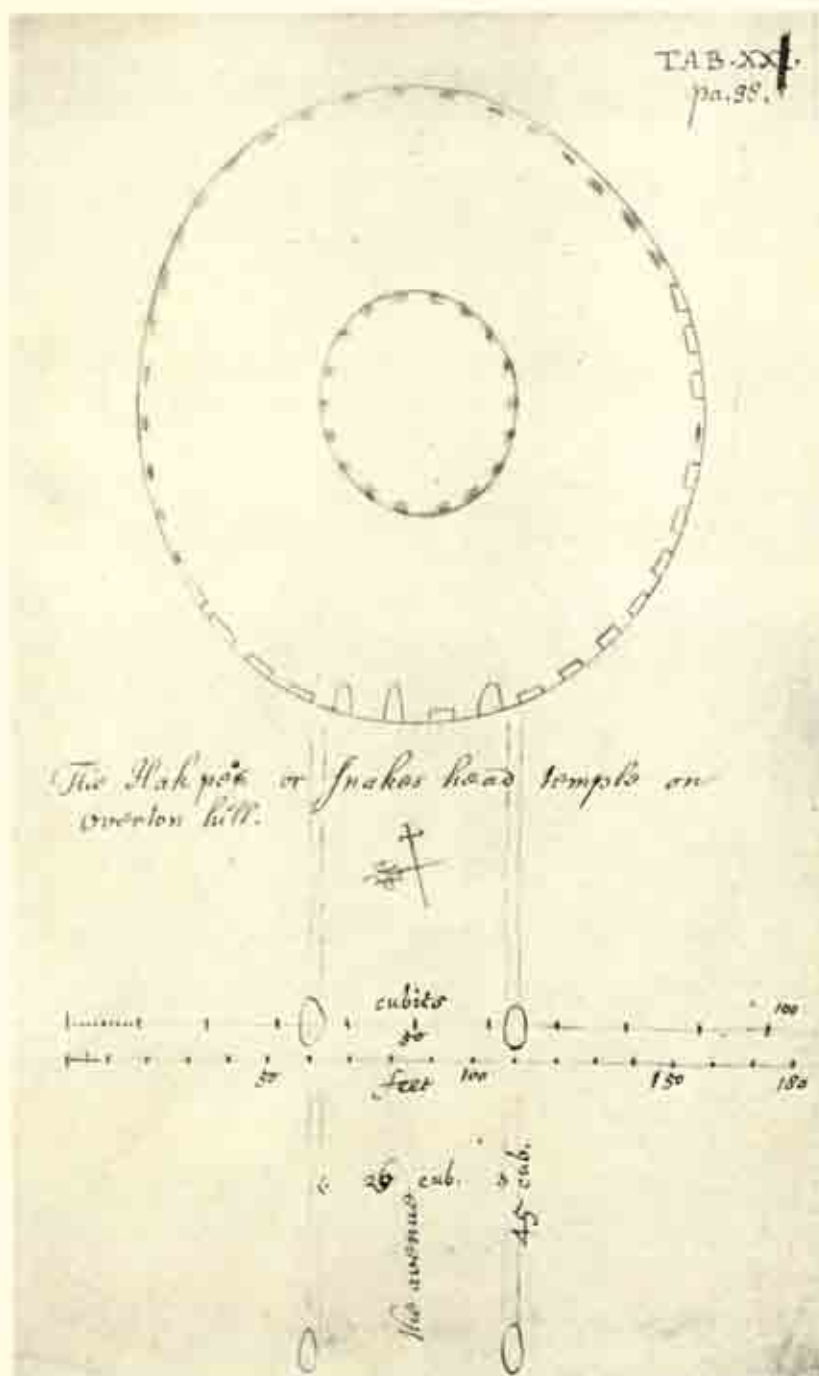
PLATE III



STURLEY'S ORIGINAL FIELD-SKETCH OF THE PLAN OF THE OVERTON HILL STONE CIRCLES. (1720)

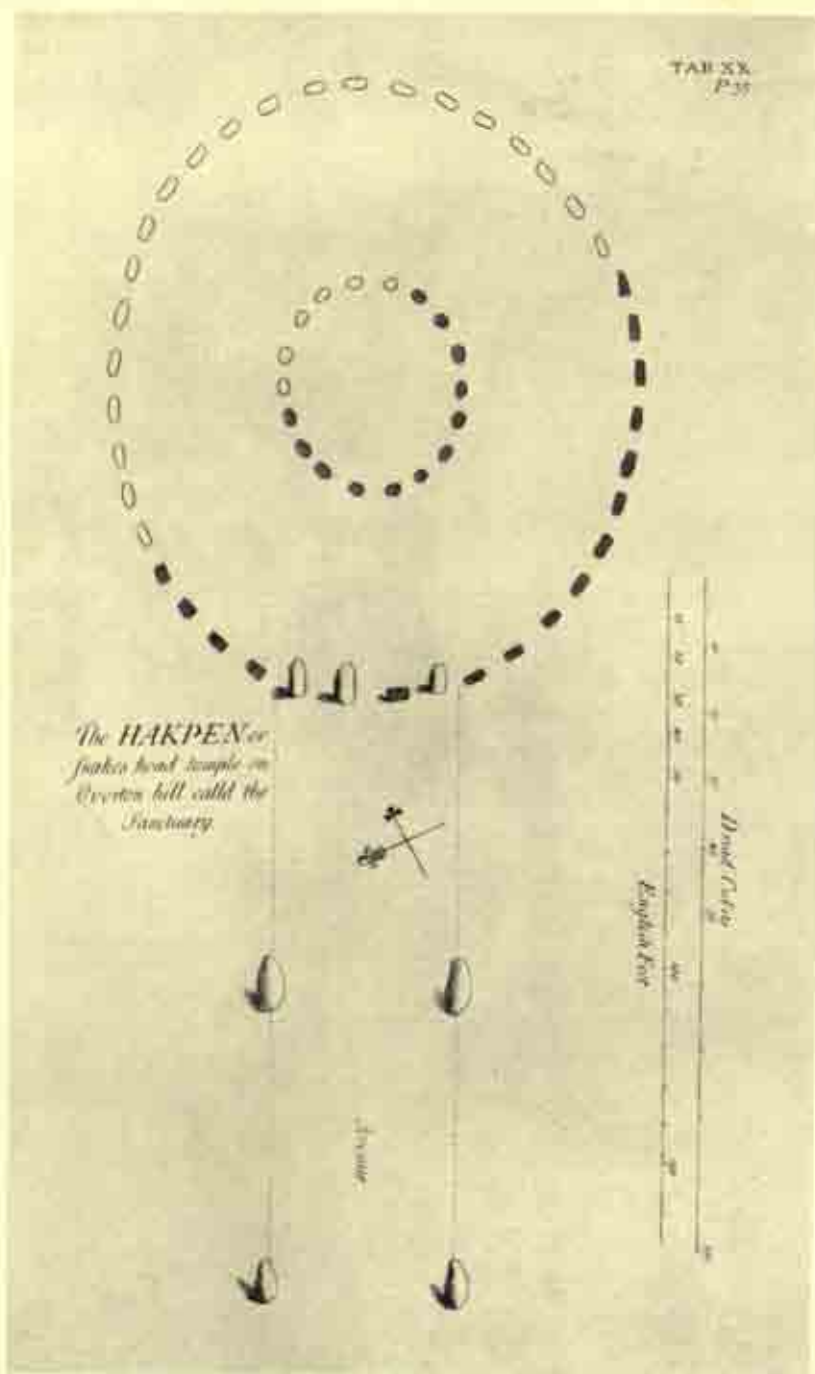
Bodleian Library, MS. Gough Maps, 231. 1. 10.

PLATE IV



DRAWING 4. (740) FOR THE PUBLISHED PLATE OF THE OVERTON HILL STONE CIRCLES, THOUGH NOT THE FINAL VERSION, SHOWING THE CIRCLES ALTERED TO OVALS.  
As in the possession of Alexander Keble.

PLATE V



OVERTON HILL STONE CIRCLES. PLAN PUBLISHED IN STURLEY'S 'ABURY', TAB. XX (1743)



## STUKELEY, AVEBURY AND THE DRUIDS

gentlemen residing in and about London, of like inclination . . . used to meet weekly, on a Wednesday evening, as a club, at the Mitre Tavern in Fleet Street. Their conversation turn'd on matters of learning, chiefly Antiquitys'.<sup>5</sup> On his coming to London in 1717 he was introduced to the club by Maurice Johnson and the next year was largely instrumental in forming the dining club into a more formal body; this in his account of its founding he calls indifferently the Antiquarian Society or The Society of Antiquaries, and under the latter title it has continued to the present day.

The foundation of the Society of Antiquaries, of which Stukeley was the first secretary, marks the real beginning of his archaeological career, during the first ten years of which he made a very considerable contribution to British archaeology. For it was between 1718 and 1725 that he carried out his monumental series of field-observations at Avebury and Stonehenge, without which our knowledge of those great megalithic structures would be materially less. At Stonehenge little destruction took place after his day, but Avebury was wrecked to such a degree that, without his record of its appearance two hundred years ago, we could glean but little from its shattered remnants. Before we come to consider his work on these monuments in some detail, it is well to emphasize one point. Since his entering Cambridge, his training and environment had been essentially that of a scientist. His medical work, coupled with a genuine bent for scientific research so far as it was known at the beginning of the 18th century, would naturally cause him to bring to bear upon the study of antiquities an acute and observant eye; a mind accustomed to diagnosis would grasp the salient points and make cautious deductions from them; while he would appreciate the value of an accurate record of fact both in words and in drawings, the latter made easier by his own considerable talent in sketching (a talent, alas, to which the engravers of his published plates rarely did justice). In fact, Stukeley was one of the first of that large band of medical men who have turned their scientific training to the study of archaeology with excellent effect.

It is commonly thought that Stukeley started his Avebury field-work, and to some extent that at Stonehenge, with a preconceived theory dominating his mind—that Hydra-headed, tortuous monster of perverse ingenuity which, in the published accounts of these monuments, is so much in evidence that the solid basis of fieldwork

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<sup>5</sup> MS. History of the Society of Antiquaries (A.K.)



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is almost stifled, Laocoon-like, by its involved coils. We shall see, however, that there is no evidence that this was the actual state of affairs.

In the newly founded Society of Antiquaries, Stukeley met and rapidly formed a close friendship with Roger Gale, son of the master of St. Paul's School. Gale had had access to, and had transcribed some of the manuscript of John Aubrey's *Monumenta Britannica*, which at that time was in the hands of Awnsham Churchill, the bookseller and publisher,<sup>6</sup> and it seems more than likely that Aubrey's description of Avebury fired Stukeley and Gale to make an expedition to this site and to Stonehenge in 1718. At all events the journey was made, and in December of that year Stukeley copied Aubrey's account of Avebury, and his plan, from Gale's transcript, into his commonplace book.<sup>7</sup>

As Mr T. D. Kendrick has shown, in the only sane book on Druids ever written,<sup>8</sup> Aubrey was the first to claim Stonehenge, Avebury and other megalithic monuments for the Druids, and it seems that from this 'humble submission to better judgment' as Aubrey himself styled his hypothesis, Stukeley ultimately built his incredible structure of fantastic theory. But in the years immediately following 1718, theory occupied a secondary place: Stukeley was working as a scientist. About this time he was attacked by gout, and rode 'on horseback in the spring, for recovery of his health. By this means, he indulged his natural love of antiquities, especially those of his own Country'. In 1721, and 1723, he undertook lengthy tours of southern and midland England with Roger Gale, making numerous notes and drawings which were published as *Itinerarium Curiosum, Centuria I*, in 1724. But his main work was the fortnight or so of each year devoted to Avebury and Stonehenge, and sufficient material remains (thanks largely to his habit of dating most of his drawings) to reconstruct a journal of his work on the former site, in which his change of mental outlook can be clearly traced.

In May 1719, he made his first 'rude general sketch' of the Avebury circles,<sup>9</sup> and part of the Kennet Avenue; in 1720 and 1721 he again

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<sup>6</sup> W. Long, *Abury Illustrated* (Devizes, 1862), 61. (Reprinted from *Wilts Arch. Mag.* iv and vii, but with additions not published elsewhere).

<sup>7</sup> Commonplace Book 1717-1748, in the library of the Wiltshire Archaeological Society at Devizes.

<sup>8</sup> T. D. Kendrick, *The Druids* (1927), 8.

<sup>9</sup> In the Commonplace Book at Devizes.



## STUKELEY, AVEBURY AND THE DRUIDS

visited the site, the second time with Roger Gale. In 1722, he made a first draft of a large scale-plan of the circles<sup>10</sup> as well as other drawings, and discovered the problematical Beckhampton Avenue.

It is in 1722 and 1723 that for the first time we can detect some hint of Stukeley's searching for a theory to account for the lay-out of the circles and avenues. On the manuscript plan of the circles of 'The Remains of the BRITISH Temple in the village of AVBURY Wilts, A<sup>o</sup> 1722' to which we have referred, the two double concentric circles within the main circle are called 'The Lunar Temple' and 'The Solar Temple'; subsequently altered to 'Northern' and 'Southern' temples respectively. On 8 July 1723, he made his well-known drawing of the stone circles of 'The Sanctuary' on Overton Hill, and in its original form called it the 'Temple of Ertha';<sup>11</sup> on 19 July he made the drawing of 'A view from the spot of the Temple at the end of Bekampton Avenue'.<sup>12</sup> His original plans of the sanctuary described below were also in the first instance called 'The Temple of the Earth'. To this year also must belong a great panoramic view of the whole Avebury complex,<sup>13</sup> although it is undated. On this drawing, probably Stukeley's finest piece of draughtsmanship, there is indicated a hypothetical circle at the end of the Beckhampton Avenue to balance those on Overton Hill, and against it is written (and subsequently heavily crossed out) 'Temple of the Infernal Regions'.

A cosmic theory was obviously in his mind, and this is amplified with regard to the circles at Stanton Drew, which Stukeley visited on 23 July 1723, after he had been working at Avebury for nearly a fortnight. The engravings made from his drawings of Stanton Drew were published posthumously in *Centuria II* of the *Itinerarium Curiosum*, where one is called the 'Solar Circle', and in his account of the circles, published in the same volume under the title of 'The Weddings', and which we know from the manuscript<sup>14</sup> to have been written in March 1724, the theory of Solar and Lunar Temples, and the Temple of the Earth, is set forth and compared with Avebury.

It is clear therefore, that when first Stukeley began to theorize, it was not his famous Serpentine ideas that filled his head, nor, in fact, does he seem to have regarded his celestial theory with any great seriousness, for he abandoned it in 1724. It is necessary to explain at

<sup>10</sup> *Avebury Drawings* (A.K.)

<sup>12</sup> *Ibid.* no. 61 (A.K.)

<sup>14</sup> A.K. MSS.

<sup>11</sup> *Proof Plates*, no. 62 (A.K.)

<sup>13</sup> *Avebury Drawings* (A.K.)



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this point that it appears to have been his custom to have his drawings engraved directly after each yearly visit, and on his next visit he would correct proofs on the spot ; increasing shading here, deleting a tree there, or altering the title. The proof engraving of what eventually became tab. XXI of his ' Abury ', (the view of the Sanctuary), was, as we have seen, originally entitled ' the Temple of Ertha ', but on 18 May 1724, when he was again at Avebury, he altered this to the sensibly non-committal ' Temple on Overton Hill ', fortunately dating the correction on the proof. A similar correction was also made on his original plans of the site. The Beckhampton ' Temple ' being no longer needed—' The Infernal Regions ' being unnecessary with no ' Temple of Ertha '—the proof of this view, mentioned above, was altered from ' Spot of the Temple ' to ' Near the Spot of the Termination of the Bekampton Avenue ' (*Abury*, tab. XXV). It is probable that the alterations to the main plan, of ' Northern ' and ' Southern ' for ' Lunar ' and ' Solar ' Temples were made at the same time.

This year, 1724, was the last at Avebury. Stukeley, unhampered by theories, completed his magnificent detailed record, both in notes and illustrations, of a monument which was being destroyed before his eyes. We owe him a deep debt of gratitude for his Avebury work, and scarcely less for that on Stonehenge which he carried on during the same years, 1718-24. Stukeley discovered the Avenue and the Cursus, and, incidentally, it is to him that we owe the term ' trilithon ' for the megalithic units at Stonehenge. The dramatic recovery of the Avenue, lost for two hundred years, by air-photography in 1921, was a vindication both of the accuracy of Stukeley's observations and of the utility of this recently adopted adjunct to archaeological research.

Any modern archaeologist who has had occasion to test the accuracy of Stukeley's field-work during the decade 1718-27, will have proved the complete reliance that can be placed upon it. In 1725 Stukeley and Roger Gale made a tour in northern England, the journal of which, entitled *Iter Boreale*,<sup>15</sup> was not published until after Stukeley's death, in the second part of the *Itinerarium Curiosum*. Mr Crawford has recently shown, in the pages of *ANTIQUITY*,<sup>16</sup> the use to which he was able to put a drawing of the stone circle of Long Meg and her Daughters, made during this tour, in identifying the site of a now destroyed adjacent circle. Instances such as this might be multiplied indefinitely.

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<sup>15</sup> The original MS. is in the A.K. Collection.

<sup>16</sup> *ANTIQUITY*, 1934, VIII, 328-9 (plates I, II).



## STUKELEY, AVEBURY AND THE DRUIDS

In February 1727, Stukeley wrote to Roger Gale :

'I begin now and then to peep over my old papers and drawings, and among antiquity matters Abury seems to touch my fancy the most at present, and probably, if business does not too much encroach upon my time, I shall publish it in a year or two'.

Had Stukeley acted on this worthy resolution his reputation today as an archaeologist would have been very different. But, unfortunately, other matters did encroach upon his time, with lamentable results. Neither his leaving London and going to live in Grantham in 1725, nor his marriage in 1728 to Frances Williamson, were likely to prejudice his archaeological judgment; but in June 1729 he took a step of which the consequences profoundly coloured his whole subsequent outlook.

Despite his scientific training, it is clear that there had always been a strong underlying vein of mysticism in Stukeley's character, increasing as the years went by. A love of elaborate symbolism and allegory probably accounts for his entry into Freemasonry in 1721,<sup>17</sup> and once he had begun to think about Druids his fancy led him into strange paths. He laid out a 'Druidical grove and temple' in his garden at Grantham, and in 1728 he buried a still-born child

... 'under the high altar in the chappel of my hermitage vineyard; for there I built a niche in a ragged wall overgrown with ivy, in which I placed my roman altar, a brick from Verulam, & a waterpipe lately sent me by my Lord Colrain from Marshland . . . there we entered it, present my wives mother & aunt, with ceremonies proper to the occasion'.

It is perhaps surprising, after this semi-pagan ritual, to find him writing in June 1729, to his friend Dr Wake, Archbishop of Canterbury, asking his advice and help in the matter of ordination for the church, and hinting darkly that his Druidical researches had led him to 'some notions about the Doctrine of the Trinity, which I think are not common'. He was apparently more explicit in a letter to Roger Gale, to such a degree that that worthy man was seriously alarmed at his friend's decision, for he wrote back urging a reconsideration. 'Your reconciling Plato & Moses', he goes on, '& the Druid & Christian Religion may gain you applause, & perhaps a Patron; but it is good to be sure of the latter upon firmer motives than that scheme may inspire people with at present'. But Stukeley was not to be deterred. He had decided that every pagan religion, particularly that of the Druids, was a

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<sup>17</sup> For Stukeley's masonic career see R. F. Gould in *Ars Quatuor Coronatorum*, vi.



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foreshadowing, not only of Christianity, but of the doctrine of the Trinity, and with this weapon he was going to battle against the sceptics in 'this age of epidemical infidelity'. And so we see him ordained in November 1729, and appointed to the living of All Saints, Stamford.

Fired with all the misguided enthusiasm of the religious revivalist, Stukeley's first task was to utilize his field-work of the last ten years as ammunition in his holy war. Poor Avebury was the first victim of this transforming process. Gale writes in June 1730 to congratulate Stukeley on his resumption of work on the Avebury book, and declares himself 'much pleased with the plan of your theologicall enlargements upon it'. Enlargements they certainly were, and in a letter in reply on 25 June, Stukeley reveals their true nature.

'The form of that stupendous work [Avebury] is the picture of the Deity, more particularly of the Trinity. . . . A snake proceeding from a circle is the eternal procession of the son from the first cause. . . . My main motive in pursuing this subject is to combat the deists from an unexpected quarter, and to preserve so noble a monument of our ancestors' piety, I may add orthodoxy'.

Stukeley had indeed plunged deeply into the waters of religious controversy. Scepticism was at this time becoming fashionable under Hume and the younger Dodwell, having developed out of the early forms of Deism of Chubb, Tindal and Toland. Now Toland, in addition to his more obviously controversial religious works, had written a diffuse and involved *History of the Druids*, containing scarcely veiled attacks on 'priestcraft' in general, which was published posthumously in 1726, and is more than once referred to by Stukeley in his published accounts of Avebury and Stonehenge. It is possible that Stukeley's attack on Deism through the Druids may have been tinged with some personal feeling against the unfortunate Toland, more particularly when we find that about 1710 Toland had published his *Origines Judaicae*, in which he ridiculed one Huetius, who in a work entitled *Demonstratio Evangelica*, had sought to prove that various Old Testament characters were allegorized in heathen mythology (including, rather unexpectedly, Moses as Bacchus)—a scheme not unlike Stukeley's own theories.

After this we are less surprised than we might be to find that, when *Stonehenge* was finally published in 1740 it was graced with a preface explaining that it, and the forthcoming *Abury*, were to be considered merely as parts of a great work entitled 'Patriarchal Christianity, or a Chronological History of the Origin and Progress of true Religion, and of Idolatry'. He had decided, he says, to publish *Stonehenge* and



## STUKELEY, AVEBURY AND THE DRUIDS

*Abury* first, 'and proceed to the speculative parts afterwards; reserving them, God willing, to the maturer time of my life'. 'My intent is', he goes on

'to warm our hearts into that true sense of Religion, which keeps the medium between ignorant superstition and learned free-thinking, between slovenly fanaticism and popish pageantry, between enthusiasm and the rational worship of God, which is no where upon earth done, in my judgement, better than in the Church of England'.

And so we find that odd and incongruous mixture in the published accounts of Stonehenge and Avebury—sound field-work and careful observation side by side with the wildest imaginative flights, according to whether Dr William Stukeley or the Rev. William Stukeley was the dominant mental character at the moment. So far as can be seen, enough remained of his scientific conscience to prevent him from materially altering the facts to fit his theories, but in one instance at all events he was guilty of a very grave crime in this direction.

His published plan (*Abury*, tab. xx) of the destroyed stone circles on Overton Hill, known as 'The Sanctuary', shows them not as circles, but as ovals (PLATE V). As Captain and Mrs Cunnington's excavations of 1930 proved, they were in fact true circles (*Wilts. A.M.* XLV, 300). After this discovery, Stukeley's reputation as a field-archaeologist seemed likely to wane, but it is fortunate that in the Bodleian there are two original field-sketches of this plan,<sup>18</sup> and in both the circles are drawn as circles, and even some of the 'extra' stones near the junction with the Kennet Avenue, the sockets for which were found in 1930, are shown (PLATES II, III). The intermediate link between these excellent plans and the misleading published record of the site is provided by an original drawing c. 1740 which, while not that from which the plate was engraved, comes very close to it in detail<sup>19</sup> (PLATE IV). In this the stones (or stone-holes) are arranged as ovals in grey wash, but are superimposed on a faint pencil outline showing them as circles, while in one of the original plans a rough oval outline has likewise been sketched. We can only feel that Stukeley, by now completely theory-ridden, thought how desirable it would be to give his snake an oval, more naturalistic, head, and so committed the serious offence of altering his original survey.

After the publication of *Stonehenge* in 1740 and *Abury* in 1743, Stukeley's archaeological career, though pursued with enormous

<sup>18</sup> MS. Gough Maps, 231, 9<sup>v</sup> 54<sup>r</sup>.

<sup>19</sup> *Avebury Drawings* (A.K.)

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vigour, is the melancholy record of the decay of a once-sound mind. It was in 1747 that he received the letter from Charles Bertram of Copenhagen, which ultimately led to his accepting as genuine the famous forgery of Richard of Cirencester, the story of which has been told in these pages by Mr H. J. Randall.<sup>20</sup> His subsequent archaeological productions were a most involved farrago of far-fetched hypotheses, coupled with an almost infantile credulity. The career of Carausius had long interested him, but unfortunately he misread FORTVNA on a coin as ORIVNA, and thereupon in a moment of inspired lunacy produced a wife of this name for his hero, announcing his momentous discovery in print in 1752. Windmill Street near Piccadilly he concludes quite rightly to have been called after an actual mill—but he goes on to assume that as windmills sometimes stood on barrows, a barrow therefore existed in Piccadilly Circus in which the king of the Trinobantes was buried!<sup>21</sup> Such instances may have an almost pathological interest to the student of mental aberrations—to those who respect Stukeley's earlier work they are pathetic.

He died in 1765 at the age of 78, being at that time rector of St. George's, Queen's Square, Holborn. His work during the ten years from 1718 shows him to have been the finest field-archaeologist that England had so far seen or was to see for a century; for the next thirty-five he was instrumental in propagating theories the very imbecility of which seems to have endeared them for ever to the public mind. Who shall apportion praise or blame to so contradictory a character?

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<sup>20</sup> ANTIQUITY, 1933, VII, 49-60.

<sup>21</sup> MS. entitled *Knave's Acre*, written c. 1760 (A.K.)



# Kells, Durrow and Lindisfarne

by F. C. BURKITT

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IN the number of ANTIQUITY which appeared in March 1934 there was an article by Mr A. W. Clapham, the object of which was to show that the Irish art which is exemplified by the 'Book of Durrow' and the famous 'Book of Kells' seems to be derived from the Northumbrian art of which the Lindisfarne Book is an example. The popular tradition, at least of the last hundred years, goes the other way: the 'Book of Kells' is popularly regarded as not only very splendid, but also very Irish. The purpose of what follows is to test and illustrate these views by the evidence of textual criticism. Most of what I have to say will be dreary to the student of art, and *crambe repetita* to the Biblical critic, but I venture to persevere, because I cannot help feeling that the text does throw some light on the illustrations.

## I

The Latin Vulgate is a revision made by St. Jerome in the year 383. So far as the Gospels are concerned it was very well received: round about 400, only 15 years from its first appearance, the new revision was officially accepted at Hippo in North Africa, and formed the basis for St. Augustine's critical study *De Consensu Evangelistarum*. But in the course of two centuries the text became much corrupted and mixed with the old unrevised texts of the 'Old-Latin'. Sometimes scribes wrote down the text known to them, substituting, for example, the Old-Latin *cotidianum* in the Lord's Prayer for Jerome's *supersubstantialem*; more often the mixture came by correcting a codex of the Old-Latin to the new revised version, but imperfectly. The result must be classed as a text of the 'Vulgate' but with Old-Latin readings occurring here and there, and such mixed texts tended to become the leading exemplars for a whole region. Besides this many small changes were made by careless scribes, and in spelling many copyists followed the style current in their own country.

Speaking generally, the texts current in the old Christianized countries were the most mingled, those introduced by more recent missionaries, such as Augustine, were the purer. The English had a Vulgate text from the beginning, but the Britons in the 5th century had only partly assimilated their Old-Latin mss to the Vulgate, as can be seen by the quotations of Gildas. Augustine had brought over codices of the



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Vulgate: the Corpus Christi Gospels (quoted as X) may be one of those he brought with him. It has a good, but not a very good, Vulgate text.

Benedict Biscop founded the monastery of St. Peter at Monkwearmouth in 674 and of St. Paul at Jarrow in 682. He was succeeded by Ceolfrid. The two foundations were only about six miles apart, and were so much under the same government that it is not even possible to know at which place the library was situated, or whether the books went from one to the other. About 678 Benedict Biscop had made a fourth journey to Italy, in the course of which he purchased some valuable books. They are described by Bede, and from his description it is clear that Codex Amiatinus (A), now in the Laurentian Library at Florence, was copied at Jarrow or Wearmouth from the books bought by Biscop and taken by Ceolfrid on his journey to Rome to present to the Pope, no doubt, as a specimen of the standard attained by his monks in the extreme limits of the civilized world.

Codex A is the leading MS of the Vulgate. Its text in the Gospels is especially valuable: there is some reason to think that it represents the text approved by Cassiodorus for his monks at Vivarium in the south of Italy, and that it was derived from a MS possessed by Eugippius, abbot of the Lucullanum (now the Castel dell'Uovo near Naples), which was believed in 558 to have belonged to Jerome himself.\* That this text should have been taken to Northumbria and survived there is a strange historical accident, but it carries with it the rider that all books with real affinity to the Gospel-text of A have a Northumbrian origin.

## II

That the famous MS known as the Lindisfarne Gospels, now in the British Museum (Cotton, Nero D iv, generally quoted as Y), is Northumbrian needs no demonstration. It was written by Eadfrith the successor of Cuthbert, and illuminated by Oethilwald, afterwards bishop of Lindisfarne (from 725). From 676 the Irish monks had gone away from Lindisfarne, and the monastery was run on the lines of Jarrow and Wearmouth. In text the Lindisfarne Gospels (Y) is almost identical with A: in handwriting and illumination it is one of the finest examples of what is commonly described as 'the Irish school of writing'. The object of this article is to determine by what right this school is called 'Irish' at all.

The three other MSS, of which I here have to speak, are the Echternach Gospels, the Book of Durrow, and the Book of Kells. The

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\* J. Chapman, *Notes on the early history of the Vulgate Gospels*, pp. 28, 41.

# KELLS, DURROW, AND LINDISFARNE

Echternach Gospels (cod. *Epternacensis*, B.N. 9389, quoted as *ept*) is a MS of the 8th or 9th century, copied, apparently from a very old exemplar, at Echternach near Trèves by an Irish monk. The readings in the margin (*ept<sup>m</sup>*) and the spellings are Irish, but the first hand of the text (*ept\**) generally supports A and Y. In Abbot Chapman's opinion, with which I agree, 'the agreements with AY against the Irish look like survivals rather than corrections'.

The Book of Durrow (Trinity Coll. Dubl. A 4.5, quoted as *durmach*) is a MS of the 8th century, decorated like the Book of Kells, but with less colour. The Book of Kells (Trinity Coll. Dubl. A 1.6, quoted as Q), is a MS of the 8th century, probably. The decorations include human figures, done with a stiffness which contrasts curiously with the beautiful freedom and accuracy of the interlaced and spiral ornaments.

## III

To illustrate the texts of AY, *ept* (with margin), *durmach* and Q, I give a few readings to illustrate their affinities. The sign „ means 'agrees with AY'.

Matt.	AY	<i>ept*</i>	<i>ept<sup>m</sup></i>	<i>durmach</i>	Q
1. II 4	sciscitabatur	„	„	„	„
2. 16	pueros	[hiat]	„	„	infantes
3. III 1	in diebus autem illis	[hiat]	„	om. autem	in illis diet.
4. 17	complacui	[hiat]	„	„	bene compl.
5. V 1	autem	„	+ ihs	„	+ ihs
6. VII 2	metietur	remetietur	„	„	remetietur
7. IX 12	medico	„	„	„	„
8. 38	eiciat	„	„	„	„
9. XII 31	spiritus autem blasphemiae	„	„	„	spiritus autem blasphemia
10. XVII 14	genibus prouolutis	„	„	„	„
11. 15	fili mei	„	„	„	filio meo
12. XXVI 17	om. die	hab. die	„	„	hab. die
13. 39a	transeat	„	+ a me	+ a me	+ a me
14. 39b	sic ut tu	„	+ uis	„	+ uis
15. 75	plorauit amare	fleuit amare	„	amarissime fleuit* fleuit amare**	amarissime fleuit
16. XXVII 44	fixi erant	crucifixi sunt (solus)	„	crucifixi erant* om. cruci durm <sup>corr</sup>	crucifixi erant
17. 49	Helias liberans eum	„	Hel. liberare eum	„	Hel. et liberaret eum. alius autem accepta lancea etc.
18. 65	custodiam	„	„	„	milites



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In this short list I have included several readings, like II, 4 *sciscitabatur*, IX, 38 *eiciat* (for *mittat*), to show that even Q preserves genuine Vulgate readings against all Old-Latin evidence. My chief object is however to exhibit *durmach* as presenting a text half-way between the pure Northumbrian of AY and the much more characteristically Irish form of Q. Wordsworth and White in editing the Vulgate decided not to use *durmach* (the Book of Durrow) regularly, regarding it apparently as a mixed text, which it is. But from the point of view of the origins of Y and Q this mixture is extremely important. Here is a MS purer in style than Q and almost equal in its elaboration, which has a purer text of the Vulgate than Q, the purer element being akin to Y (and A). Does it not suggest that as its text is akin to Northumbria, so also its decoration is akin too? What is more, this Northumbrian text was itself a stranger, imported by Biscop from South Italy. Its adoption, or partial adoption, by the scribe of Durrow suggests that he may have been rather an innovator in art than a perfecter of an immemorial native tradition.

### IV

The actual history of MSS cannot be entirely inferred from their text alone. Perhaps my view about these famous Gospel codices, Lindisfarne, Durrow, and Kells, can best be stated in a few paragraphs of conjecture.

The history of Lindisfarne (Y) we know. In the late C. H. Turner's opinion it was actually copied from a MS, of which a fragment survives in the Durham chapter-house library (A II, 17, foll. 103-11). In any case it is the brother of Cod. Amiatinus, the parent of which was brought by Benedict Biscop from near Naples. There is nothing 'Irish' about it, except on the supposition that its art must be called Irish.

Durrow (*durmach*) is very much like Y. Its text is not pure Northumbrian, but very much like it. Its immediate ancestor must have been corrected to the AY text. I have an impression that it was not like *ept*, an AY text which has suffered Irish contamination, but the other way round. In an inscription found in it the scribe says *qui hoc scripsi . . . euangelium per xii dierum spatium*. No one could have written the Book of Durrow, with its beautiful spirals and interlacings in this short time, but it would suffice to correct an Irish text to near the AY standard.

Kells (Q) has an 'Irish' text, like DELR and *gat*, that is to say a Vulgate text, much contaminated by inferior readings of uncertain



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origin. But Q is not absolutely bad. It does often side with the best MSS, and the spellings *Moses* (for *Moyses*) and *Caiaphas* (for *Caifas*) do rather suggest Northumbrian influence. Just about the time when Q was written—it was doubtless written at Iona itself—Roman customs were coming in at the great Scottish (*i.e.* Irish) monastery. Adamnan had already adopted the Roman tonsure before 700. The nearest source for Roman ways was Northumbria; among such ways may fairly be counted large-size human figures, which are found in Q, but not in *durmach* or in Y. There is nothing to suggest that Q was written in Ireland itself; no doubt it was taken from Iona to Kells, where the monks retired when they deserted Iona.

### V

One final detail may be noted as an indication of Northumbrian (*i.e.* Italian) influence in Q. Greek MSS are written with the upright strokes thick, the horizontal strokes thin. This is done by keeping the right elbow close to the body, as we were taught by governesses. But Latin scribes inclined to stick their elbows out, and this was also done by unfettered folk like the Irish, who stuck their elbows out still more. Consequently real Irish MSS, like the Book of Armagh or the Book of Mulling, have their upright strokes thin, and any thickness comes in horizontal or slanting strokes. Now Y, *durmach*, and Q, all have their downstrokes thick, and therefore their scribes' right elbows were brought in close to their sides. I cannot help believing that they had been taught to write in the classical way, the way that both Celts and Teutons have always in their hearts so disliked.

# The Flint-Knapping Industry at Brandon

by RAINBIRD CLARKE

DESPITE the obvious analogies between the ancient and modern flint industries of Britain and the equally obvious gaps in the continuity of their evolution, neither archaeologists nor economic historians have stooped to elucidate the problems of what it has become a platitude to call 'The World's Oldest Industry'. The flint-knapping industry still existing at Brandon for the production of gunflints has alone among the flint-knapping industries of Britain been described with any degree of adequacy. A bare record survives of the former existence of a score of similar industries but the majority is unrecognized and neglected. The student of the question today has as much cause to bemoan the paucity of data as did Beckmann, who wrote in 1814 'Many of my readers will perhaps be desirous to know in what manner our gunflints are prepared. Considering the great use made of them it will hardly be believed how much trouble I had to obtain information on the subject'.<sup>1</sup> The magnitude of the dumps of waste products at Brandon at the close of the Napoleonic War provoked exclamations of wonder from contemporary topographers<sup>2</sup>, but apparently the first to realize the interest and significance of the Brandon industry was Dr J. Mitchell, who in 1837 communicated his observations to the *Edinburgh New Philosophical Journal*.<sup>3</sup> In 1870 James Wyatt, F.G.S., contributed a chapter on the subject to Stevens' *Flint Chips*, but the classic account from which all subsequent articles derive is that by S. B. J. Skertchly, of H.M. Geological Survey. His monograph, written in 1876 and published by the Survey as a special memoir in 1879,<sup>4</sup> with all its faults describes the technique of the industry in

<sup>1</sup> *History of Inventions*, 2nd edition, iv, 611. Writing on the subject in his 'Cyclopaedia' in 1819, Rees mentions French sites by name but preserves a politic secrecy with regard to Britain, mentioning no single site. (See article 'Gun Flint, in *Technological Mineralogy*', vol. xvii).

<sup>2</sup> *Excursions in the county of Suffolk*, 1818, I, 99.

<sup>3</sup> xxii, 36-40.

<sup>4</sup> 'On the Manufacture of Gun Flints', 1879. (District memoir of Geological Survey of England and Wales).



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greater detail than any other publication. The almost complete lack of documentary records (the ledger book of the Brandon Gunflint Company from 1838 to 1848 and two books of accounts for 1806 are the exceptions)<sup>\*</sup> and the entire absence of archaeological excavation have succeeded in obscuring all but the most recent stages in the evolution of the industry. In other of the flint-knapping industries our knowledge is confined to the record in some local directory of the cessation of their operations, or to the discovery on the surface of undated waste products or, more rarely, of finished gunflints and strike-a-lights, debris of an extinct industry.

No attempt will be made in this article to give a minute description of all that is known of the knappers of Brandon, their history, their methods of work and their products. It will consist of a short historical introduction explaining the relation of the Brandon industry to similar industries elsewhere and considering Skertchly's claims for the continuity of the prehistoric and modern flint-working at this site. This will be followed by a technical description of the flint mines and the processes of the manufacture of gunflints with brief notes on the numbers of the knappers, their vocabulary, their output, the prices and markets for the finished article, and the by-products of the industry.

The distribution of flint industries is controlled primarily by the geological occurrence of the mineral, and secondly by the existence of a population requiring flint for building purposes. The relatively smaller bulk of strike-a-lights permitted an increasing divergence between the market and the site of the manufactory. When flint was required for building it was more often the knapper than his material that was exported. Even now the Brandon knappers migrate to the scene of their labours, using local material for their purpose. It is impossible to plot the distribution of the strike-a-light industries of Britain, for the introduction of the gunflint effected a complete re-orientation and obscured the earlier development of the industry. It would be incorrect to regard all the gunflint industries of which we have knowledge, as the descendants of those strike-a-light industries which existed at the time of their transformation, for in East Anglia at least, several industries can be shown to be late offshoots from Brandon in the heyday of the gunflint manufacture. It is probable that every manufactory of strike-a-lights having access to flint of the requisite quality, that is, homogeneous,

<sup>\*</sup> The first in the possession of Mr F. Edwards of Brandon to whom the writer is indebted for this information, and the others in the possession of Mrs Snare of Brandon. He also wishes to express his indebtedness to another knapper, Mr V. Edwards.



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free from fossils, and almost lustreless when fractured,\* soon engaged in the lucrative business of gunflint manufacture when this branch of the arms industry developed.

Yet the manufacture of strike-a-lights continued alongside that of gunflints, for the latter are to some extent a substitute for the former, but the gunflint remained the senior partner in the combine. The manufacture of gunflints is recorded from Turkey, Egypt and Morocco, from Albania, Sicily, Italy (Vicenza), Holland and Spain (Galicia). At first rough pieces of convenient size were fitted in flint-locks, then with the general use of this fire-arm by the beginning of the 18th century a demand arose for more precise and durable flints. This bred a demand for the skilled labour essential to their production. It is thus in 1727 that we find the Hanover War Chancery sending workers to England to learn the trade. In Germany the troops of the Duke of Brunswick first used flint-locks in 1687, a year later than the English forces which adopted it in the year of Monmouth's rebellion. In France the first regular manufactory was established in 1719 though flint-locks were already in use. In the 18th century the flints from Champagne and Picardy were so excellent that a ban was placed on their export. At this time the Dutch are seen purchasing vast stores of flints and drawing the customary large profits in their capacity of arms racketeers. More is known of the processes of French gunflint manufacture than that of any other site except Brandon, and the parallels and divergences are equally remarkable. They will be considered in discussing the problem of continuity at Brandon. Eight hundred men are said to have been employed in the manufacture at Lye, Couffy, Meunes and Noyers in the year 1800. As late as 1870 gunflints were made at Romantin, St. Aignan, Lye and Meunes, the majority at the last named site.

In Britain the cretaceous formations of East Anglia, the Downs and Salisbury Plain account for most of the known sites of gunflint manufactories. In Suffolk those at Cavenham, Tuddenham, Mildenhall (near Wild Street) and Icklingham are probably to be regarded as outliers of Brandon. Indeed the superior quality of the flint at Icklingham caused the knappers to abandon Brandon from about 1846 to 1850, although they returned to Brandon at the end of each week. When difficulties arose over stone-raising all the knappers retired to Brandon save one who continued till 1874. The sites in Norfolk at Weeting

\* For microscopic photographs of flint sections, see Report of Smithsonian Institution, 1897, part 1, plate 16.

# PLATE I



LINGHEATH FLINT-MINES, BRANDON, SUFFOLK.

The flint-pits have been worked for over two centuries. They are sunk fairly close together, and each is surrounded by its tip of excavated chalk. Aisle, the last surviving miner, is standing by a heap of nodules extracted from the adjoining pit.

Ph. Dr. E. Gerd Carpent.



PLATE II



LINGHEATH FLINT-MINES, BRANDON

Adley, with his peculiar pick. The pick, hammer, spade and crow-bar the only tools used.  
PA. Dr. E. Cecil Curwen.

PLATE III



FLOORSTONE "IN SITU" AT END OF "BURROW", LINGHEATH FLINT-MINE, BRANDON  
*Ph. Randall Clarke*



PLATE IV



FLINT MINER'S PICK AND HAMMER, LINCHEATH, BRANDON  
*Ph. Reinhold Clarke*

PLATE V



LINGHEATH FLINT-MINES, BRANDON

The mouth of one of the pits from which Ashley has brought up a block of flint. No ladder or tackle is used.  
Ph. Dr. E. Cecil Curwen.



PLATE VI



BRANDON FLINT-KNAPPING WORKS

Edwards quartering a block of flint, i.e. breaking it into smaller pieces suitable for striking flakes from. A heavy steel hammer is used and a leather knee-pad

Ph. Dr E. Cecil Curwen

PLATE VII



EDWARDS-STRIKING FLAKES FROM THE EDGE OF ONE OF THE 'QUARTERS', THUS PRODUCING A CONE-CORE. NOTE THE TWO FLAKES DETACHED

*Ph. Dr E. Cecil Curwen*

PLATE VIII



FRED SNARE KNAPPING FLAKES ACROSS AND GIVING A SECONDARY TRIMMING,  
CONVERTING THEM INTO GUN-FLINTS.

*Ph. Dr. E. Carl Curwen*



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(Botany Bay) and Catton<sup>7</sup> on the outskirts of Norwich owed their existence to emigrants from Brandon, as did in all probability that at Whitlingham where, it was declared in 1887, they were made until recently.<sup>8</sup>

The manufacture of gunflints at Thetford is doubtful but flints from Brandon were counted and stored there about 1880. In Essex there were workshops at Gray's Thurrock and Purfleet, where it ceased apparently about 1837. In Kent there was activity at Crayford, Chislehurst (during the Peninsular War it is alleged),<sup>9</sup> Greenhithe, Northfleet,<sup>10</sup> Lewisham and Maidstone where the quarries contained only one stratum of suitable flint. In south Wilts intermittent operations were carried on at an open air flaking and knapping site at King Manor, Clarendon, near Salisbury, though there seems to have been no regular manufacture.<sup>11</sup> Dr R. C. C. Clay has published a site at Broadchalk, Wilts.<sup>12</sup> He regards the implements published by the Rev. H. G. O. Kendall<sup>13</sup> as of Iron Age date as being gunflints. This would add to the company of gunflint manufactories Butser Hill, Hants, and Figsbury (inner ditch),<sup>14</sup> Laverstock Down, Hackpen Hill, Dean Hill, and Juniper Down, in Wilts, and other sites. Mr R. H. Chandler has published three sites near Eynsford, Kent, and furnished a similar explanation.<sup>15</sup> An isolated industry is at Beer Head, Devon (undercliff on the west side) where the stress of the Napoleonic War led to the production of gunflints from material obtained from a landslip. This ceased before 1835. At Glasgow gunflints are also said to have been made from imported stone.

Dazzled by the revelations of neolithic methods of flint mining revealed by the excavations of Canon Greenwell at the neighbouring site of Grimes Graves, Skertchly may be pardoned for assuming continuity between the prehistoric mining and knapping industry there and the similar modern industry in the same area. Before examining his conclusions it is necessary to set out his train of argument. He traced an evolution from the discoid scraper with bulb of percussion regarded by him as of Neolithic date, to the Old

<sup>7</sup> Ceased, c. 1878-9. H. B. Woodward, *Geology of England and Wales*, 2nd ed., 420.

<sup>8</sup> This site is probably identical with that at Trowse listed by W. G. Clarke, *Norfolk and Suffolk*, 1921, 120.

<sup>9</sup> W. Johnson, *Folk Memory*, 1908, 231.

<sup>10</sup> Extinct before 1837.

<sup>11</sup> Wyatt, in *Flint Chips*, 1870, 588.

<sup>12</sup> *Antiq. Journ.*, 1925, v, 423-6 (5 figs.)

<sup>13</sup> *Antiq. Journ.*, 1925, v, 158-63.

<sup>14</sup> Ceased over a century ago.

<sup>15</sup> 'Some supposed Gun Flint Sites'. *P.P.S.E.A.*, 1918, II, 360-5.



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English strike-a-light and thence to the modern gunflint and strike-a-light. According to this view the gunflint was only a modification of the present strike-a-light. The modern square form of strike-a-light was introduced from France and is known as the 'Frenchman'. He further stressed the similarity of the round-headed English iron flaking-hammer, superseded by the French hammer only at the end of the 18th century, with prehistoric hammer-stones. Both ancient and modern knapper undercut the sides of their implements. In the sphere of mining Skertchly noted a similarity between Grimes Graves and the shafts now worked at Lingheath near Brandon in 'several remarkable particulars'. He emphasized the point that at both sites a number of pits were sunk close together to exploit the 'floorstone'. There existed too an additional parallel in the method of working. Both drove burrows about 36 feet long into the chalk and both 'drew' the flint in semi-circular apses and when the shaft was exhausted filled the old workings with chalk. After referring briefly to the use of a hollowed cup of chalk as lamp, and now as candle-stick holder, Skertchly plays his last card with a touch of triumph, pointing to the obvious descent of the modern one-sided pick of iron used at Lingheath from the miner's antler in the Neolithic shafts. He added that this pick was made only at Brandon as are all the tools of the modern flint-knapping industry.

This formidable argument for the continuity of flint working and flint-mining at or near Brandon attracted the attention of the anthropologists, and in 1879-80 the Anthropometric Committee of the British Association reported on the physical characteristics and racial affinities of the inhabitants of northwest Suffolk. It was declared that 'several portraits from that locality have been mistaken by competent judges of physiognomy for Welsh. The inhabitants contrast strongly in the colour of their hair and eyes with the population of other parts of the country'.<sup>16</sup> Very dark hair and eyes evidently predominated. When however it came to inferring that the technical vocabulary of the modern miners and knappers provided an intimate link with 'pre-Aryan language' a re-examination of the facts was obviously desirable.

At the outset it may be stated that Skertchly's typology of strike-a-lights and gunflints was formulated in the absence of any but the most nebulous links between Bronze Age scrapers and extremely modern specimens of the classes mentioned. Such a morphological sequence

<sup>16</sup> Reports (J. P. Harrison), 1879, 400-1; 1880, 158-9, 626-7. See also J. Beddoe, *Races of Britain*, 1885, 254.



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is devoid of value in the absence of accurately dated intermediate types. Skertchly's claims in this respect assume an even more doubtful air in view of the radically different genealogy propounded by the late Fred Snare, a knapper celebrated for his knowledge of the history of his industry. He controverted Skertchly's view that the modern gunflint and strike-a-light were collateral descendants from Old English strike-a-lights by pointing out that those of the straight-heeled variety of horse-shoe pattern are not English but a French type still made on the Continent in 1908 and sold in Valencia.<sup>17</sup> In his opinion the strike-a-light of the 16th century was rudely oval in shape, evolving into a circle and later into a square preserving a slightly uneven curve at the sides. From this the square gunflint was born. In his comparison of the ancient and modern mines Skertchly has been guilty of selecting his evidence. From the most cursory examination a fundamental contrast is apparent. The minute shaft of the modern mine with its prominent stagings and its crisp angles bears little superficial resemblance to the broad open stageless pits at Grimes Graves. In addition no reference was made to the crooked galleries in the Neolithic mine and the straight burrows at Lingheath, or to the relative position of the 'floorstone' to the galleries which are situated above this stratum at the former and below at the latter site. At Grimes Graves no chalk was removed from below the flint bed. Further, at Lingheath the knapping sites are not in that close proximity to the mines apparent at Grimes Graves. When all due subtractions have been effected from the volume of evidence adduced by Skertchly in favour of his thesis it must be admitted that the balance of probability is still in his favour. This conclusion is reached after a consideration of (1) the French knapping industry; (2) the continuity of the use of flint; (3) the anthropology of the knappers.

A study of the modern French mining and knapping industries can be commended to those who would argue that the analogies between ancient and modern flint-working at Brandon are fortuitous, resulting from the geological similarity of the mining sites and the physical similarity of the flint. In respect of all the parallels noted by Skertchly at Grimes Graves and at Lingheath the French practice is dissimilar. Further the square-headed French flaking-hammer is in contrast to that formerly in use at Brandon, while the practice of undercutting gunflints is unknown, the 'heel' being chiselled and not cut.

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<sup>17</sup> W. Johnson, *Folk Memory*, 1908, 190.



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The continuous production of flint tools till the Iron Age and possibly later is a commonplace. In East Anglia the squared flint facing of the town walls at Caistor-by-Norwich and St. Albans, and at the Saxon Shore fort at Brancaster among others, testify to continued activity in the Roman Age, while strike-a-lights in Saxon graves<sup>18</sup> carry on the story to the Middle Ages when a thousand churches rise with rich flush work ornamentation. In all probability the intricate geometric designs of the church of St. Michael's Coslany, and the precision of the Bridewell facade at Norwich are the handiwork of Brandon knappers. The strike-a-light and the gunflint bridge the gap with the 20th century.

In the sphere of anthropology may be noted a curious restriction of the practice of both mining and knapping to a few families, while intermarriage between these groups is more than common. The five generations of knappers in the ancestry of one of the modern knappers may account for his unusual skill.

To sum up, the evidence for a continuity of flint-mining and knapping at or near Grimes Graves and Brandon is strong, though not so conclusive as Skertchly asserted. Many gaps must needs be bridged, many stages in the evolution explained, before the homogeneity of the industry can be admitted. Meanwhile it may be conceded that Brandon enjoys and exploits a pedigree as convincing as any product of genealogy and can rest assured that only the spade can dethrone it from its high estate.

The raw material for the Brandon industry is now obtained from chalk and stone pits within a 40 miles radius. Only a small proportion is derived from the peculiar flint pits on Lingheath,  $1\frac{1}{2}$  miles southeast of Brandon. Since about 1720 Lingheath has been the predominant, though not the exclusive, source of supply for the Brandon knappers. Mining persisted till the late 19th century at Santon Downham<sup>19</sup> and Icklingham<sup>20</sup> in Suffolk and at Weeting in Norfolk. The Icklingham pits were worked till about 1874 though none were open in 1876. Their number is estimated at 500, and they are situated in a circumscribed area on Seven Trees Breck about  $1\frac{1}{2}$  miles north of the village. Icklingham and Lingheath mine-sections are similar. On Bromehill, Weeting, south of Grimes Graves, deep pits were sunk reaching the

<sup>18</sup> e.g. at High Down, Sussex. Evans, *Ancient Stone Implements*, 1897, 314.

<sup>19</sup> Near the main Thetford-Brandon road.

<sup>20</sup> Section in Geological Survey Memoir: *Parts of Cambridgeshire and Suffolk (Ely, Mildenhall and Thetford)*, 1891, p. 45. To save expense of cartage the last knapper, Ashley, quartered and flaked stone at the mine-head.



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'floorstone' at 35 feet. There are also pits at Santon, Norfolk<sup>21</sup> and in and around the Elms Plantation, Brandon Park.<sup>22</sup> Skertchly suggested that the latter were worked out before the Lingheath pits were sunk and are possibly earlier than the gunflint trade. The source of supply for strike-a-lights is unknown, but it is possible that the shafts of older pits have been overwhelmed by blown sand.<sup>23</sup>

Sandy Lingheath is honeycombed with pits from Brandon Park on the west to the slopes of the Little Ouse Valley in the east (PLATE I).<sup>\*</sup> The shafts now open are in the southeast corner of the area. When the pits near the river, known as the 'Fleet' pits, were exhausted, mining operations followed the flint seam southwards and westwards to the heath, the pits becoming ever deeper. On Lingheath today hundreds and perhaps thousands of crescent-shaped mounds of grey and disintegrated chalk dominate the landscape. In June 1934 two pits were open—one was exhausted and a fresh shaft was being sunk by the last flint miner, Ashley (PLATE II). With this attenuated labour supply it is obviously inequitable to regard as typical all the conditions of contemporary mining. Consequently a generalized description of the conditions of mining prevailing during the last century will be given although the account of the construction of the shaft is based on the measurements of the present writer in June 1934. Where a divergence occurs Skertchly's figures will be quoted.

A pit is begun by excavating sand and gravel from an area 10 feet long,<sup>24</sup> 3 feet broad and the same deep. The miner then digs down 5 feet in the centre, producing a cavity 5 feet by 2 feet at the bottom of this stage by undercutting the sides. A further stage is left at every 5 feet 6 inches, alternate stages being orientated north and south and those intervening east and west. The mouth of the shaft is oriented so that the last stage or 'Two' shall face the midday sun. The depth of the stages varies from 5 to 6 feet, the two exceptions being the first and last of the series, the bottom stage sometimes attaining 9 feet. The dimensions of the stages are controlled by the necessity of admitting

<sup>21</sup> Shallow shafts only 13 feet deep near Blood Hill tumulus, and in the eastern arm of the former Half Moon plantation.

<sup>22</sup> About one mile north of Shaker's Lodge.

<sup>23</sup> The classic example of a sandstorm for this district is described in the Geological Survey Memoir cited, p. 89, and in *Phil. Trans.* 1668, no. 37, 722-4 (T. Wright) (abridged 1809, II, 264-5).

<sup>\*</sup> We are much obliged to Dr E. Cecil Curwen for the photographs which he permits us to reproduce.—EDITOR.

<sup>24</sup> Nine feet is more usual.



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the miner's body and of raising the flint to the surface. In comparison with Neolithic mines they show a tendency to reduce to a minimum the 'dead work' involved. The average dimensions of the shaft are 3 feet by 2, and the depth in this case from the surface to the bottom of the burrow or gallery was 43 feet 6 inches.<sup>25</sup> The sides of the shaft are undercut to the extent of about 3 feet in every 15 of depth. This sinking 'on the sosh' as the miners term it, is to protect them from the weather and to prevent accidents from falling stones.<sup>26</sup>

At the bottom of the shaft the last stage is extended 3 feet horizontally and then the main burrow is driven at right angles for about 36 feet, terminating in an apse called a 'draw'. The burrow is 7 feet wide and 2 feet 3 inches in height, while the apse measures 12 feet in diameter with a maximum of 2 feet 6 inches vertically. The next shaft will be sunk so that it pierces the roof of this apse.<sup>27</sup> The geology of the mine can be seen from FIG. 1. Pits communicate only when the flint is good. This section may be regarded as standard except that an additional stratum<sup>28</sup> or 'sase' of flint termed the 'Upper Crust'<sup>29</sup> occurs in other pits between the Topstone and the Wallstone. Formerly trial pits were sunk 10 feet below the floorstone to a stratum of 'rough and smooth blacks'. The former were fit only for 'common' gunflints, but the latter were of good colour, clean cutting and of excellent running quality. They occurred too sparingly to be remunerative and difficulties with water led to the abandonment of the enterprise. The seams of flint are irregular nuclei rather than continuous layers, and in consequence one or more of these strata may be absent. The flat bottomed floorstone (PLATE III) is always burrowed for and is generally but not always continuous. When over 1 foot thick it is grey in the middle. The Wallstone, always continuous, is of good quality, being generally black though sometimes it is grey or spotted. It is burrowed from above. When present, the Upper Crust is taken out only in excavating the shaft and is not burrowed for unless in great demand as building stone. The toppings are burrowed from beneath in filling in the pit. Flint is extremely variable in quality and in constancy of colour. Some flint quite black *in situ* may crack and change to a milky tint in the course

<sup>25</sup> Skertchly's shaft was 37-38 feet in depth.

<sup>26</sup> In this connexion we cannot forbear quoting the miner's remark that he 'never sank stunt, but under-ran his two by bubber-hutching on the sosh'.

<sup>27</sup> Skertchly's ground plan of a mine is idealized.

<sup>28</sup> 'Horns' occur also occasionally below the Wallstone.

<sup>29</sup> Not in the section shown in FIG. 1, opposite.



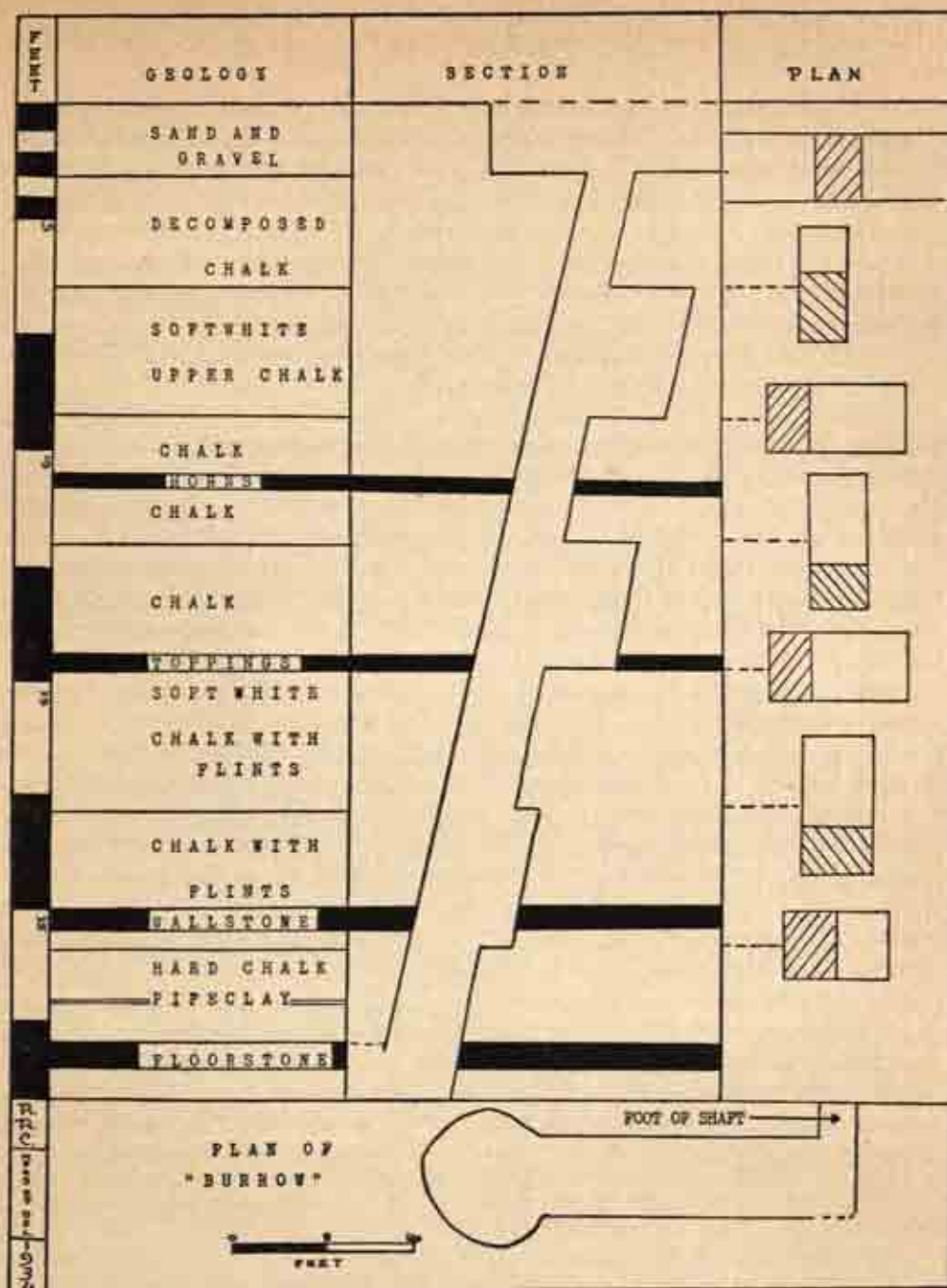


FIG. 2. DIAGRAM SHOWING STRUCTURE OF MODERN FLINT-MINE, LINGHEATH, BRANDON

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of an hour, owing to sun and wind, while other flint is unaffected by exposure to weather. An average time for digging a pit of this depth is one month, and if the flint is of fine quality will take six to nine months to exploit. Only a portion of the excavated material is brought to the surface, the flint and chalk from the first 'draw' being raised. The chalk is heaped at the head and the flint at the foot of the pit. When the pits intercommunicate the fine material from the new burrow is deposited in the old.

The flint miners or stone diggers use the peculiar one-sided pick already referred to, a heavy iron hammer, a shovel and a short crow-bar (PLATE IV). The pick is used to cut the chalk and clear it away from the stone. The iron hammer weighing 5 to 7 lbs. is employed to break up the flint into blocks weighing from 2 to 16 stones. The overhanging sides of the shaft, which can be compared to a well, a chimney and a newell stairway, prevent the sky from being seen at the bottom, and operations are carried on by candle light. No windlass rope, pail, or ladder is employed in these pits (PLATE V). The miner himself ascends and descends by a series of footholes or 'toes' in the chalky side intermediate to the stages. The flint and chalk are raised three stages at a time upon the head of the miner, who formerly wore the crown of an old bowler hat to protect him. When the miners were more numerous one was stationed at each stage and the flint was passed from hand to hand. The reasons for this non-division of labour are probably the speculative nature of a flint-mine and the absence of sufficient capital for expensive plant. The pits tend to become rounded by the obliteration of the angles. This is accentuated by filling in the shaft with chalk. Formerly when the workable flint was exhausted, the miner received a shilling from the Trustees of the Poor of Brandon, owners of Lingheath, for this labour. In 1908 it was said that the practice of filling in old shafts was in desuetude. A digger can stake out a claim for one pit, begin to excavate another and exploit a third. The miner is his own master. He pays to the Charity Commissioners of the town a royalty or 'groundage'<sup>20</sup> on each plot or 'take' where a pit is sunk. This charge is transferred to the buyer of flint. The excavated flint is measured in 'jags', which is the quantity piled on the ground so that when standing on one side the bottom of the flint on the other can be seen. One and a half jags are equivalent to a horse load.

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<sup>20</sup> Wyatt, in error, denies the existence of this royalty. In 1837 Mitchell records the miners' complaint against the rent of 5s per jag.



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The flint is carted from Lingheath to the knappers' workshops near their flint-faced houses in the flint-walled byways of Brandon. The first operation in the manufacture of 'gunflints' is termed 'quartering' (PLATE VI), the dressing of the rough flint preparatory to flaking. The knapper, wearing a leather apron, places on his left knee, protected by a tightly bound leather pad 6 inches by 12, a large nodule of flint which may vary in weight from a quarter to nearly two hundred weights. If the weather is damp the moist flint is first dried round a fire. A large or small hammer is employed according to the size of the nodule. The first or large quartering-hammer is hexagonal in section. Its composition is iron, faced with steel and weighs about 5 pounds. The second or light hammer weighs about 3½ pounds. Worn hammers are preferred. The knapper taps the flint at a suitable line of cleavage, and by the sound knows where to strike. If the hammer falls dull and jumps, the stone is double-coated or of mixed colour beneath the coat. If the stone is cracked, it flies to pieces. If on the other hand the coat is hard and the flint sound the hammer rings. The nearer the crust the better the flint. The blow is given from the elbow and the hammer is allowed to fall from above on the natural upper crust. No force is used. It is essential for flaking that a square edge be produced. There is no regularity in the pieces struck off but they average about 6 inches square.<sup>31</sup> A small 'old English' hammer is used for squaring flint for building purposes.

The second and most difficult process is flaking (PLATE VII). For this, an oval hammer of native design was formerly employed until it was superseded by the French flaker now in use. Diverse legends as to the date and manner of its introduction are in circulation, but the most probable is that it was introduced by French prisoners of war at the time of the contest with Napoleon, when Brandon supplied the whole British army with gunflints.<sup>32</sup>

In order to produce flakes of the required shape and quality the stone must be struck at the proper angle at the exact spot with the

<sup>31</sup> Mitchell, 1837, terms the quarterer a 'cracker'.

<sup>32</sup> Mitchell, p. 38, alleges that the French mode was introduced into England by James Woodyer, of Kingsdown between London and Maidstone, who died c. 1780-90. Probably French methods reached Brandon some time after their use in Kent. Skertchly's legend is that the flaking-hammer was introduced from France, probably during the war of the Spanish Succession by a prisoner called Péro. By 1927, the version had it that the name of the incarcerated knapper was Freuer who, at the conclusion of hostilities returned to Brandon, married, and founded the family of knappers, the Frewers. (Rogerson, *Blackwood's Magazine*, April 1927, p. 528).



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correct force. A large or small hammer is used according to size. The heavy hammer can strike longer flakes than the light one. The longitudinal section of the steel flaker is rhomboidal. The centres of the sides are flattened to strike off irregular projections on the quarters. The faces measure  $\frac{1}{2}$  by  $\frac{1}{8}$  inches and the whole weighs from  $1\frac{1}{4}$  to  $3\frac{1}{2}$  pounds, according to its condition and size. When worn the faces are filed down. The tapering shaft enters a hole of such slight diameter as to prove its unsuitability for delivering a heavy blow without breaking. The object of the hammer is not heavy percussion but the concentration of the greatest possible weight with the least possible volume. The handle is not for momentum but for precision in striking. The quarter is held against the leather knee-piece at an angle of  $45^{\circ}$ . The hammer is raised 2 inches from the elbow and light blows are dealt to the surface. If the flake is to be thin the blow falls just inside the face of the hammer, if thicker slightly further in. The outside flakes, 6 inches long, an inch wide and with two ridges are removed, leaving a rough polygonal shape. The next set of flakes are struck a little to one side of the previous set. The flakes run so as to include the angles or ribs of two of the first flakes and are thus double-backed. The perfect flake should have a flat face, even edges and parallel ribs, but this is possible only with good stone. Single-backed flakes are never struck intentionally, but unless flakes were of different sizes much waste would ensue. The flaker works round the quarter, removing two or three rows according to quality. Only the flaking hammer now in use can produce double-backed flints, the old English hammer producing only ribless flakes. If the flake is of good quality it is dropped into either the 'best tub' for double-backed or into the 'common tub' if it is single-backed. If defective it is cast into the chip tub and eventually goes to swell the heap of chips outside the workshop door. The cores are relegated to the 'builder tub'. Formerly a fifth tub the 'little-untub' was used to contain small flakes for apprentice boys to practise on.

The last stage in the manufacture is the conversion of the flakes into gunflints by knapping (PLATE VIII). The knapping hammer is an old cast steel file 9 inches long, and half an inch by a quarter inch in section. This requires tempering and the edge must be kept square by filing and often only lasts a fortnight. The knapper sits at an angle to a wooden block, generally the bole of an oak, in which a stake of soft iron 6 inches in length and 1 inch square at the shoulder is embedded about 4 inches from the edge. This stake is set slightly oblique and



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is surrounded by leather to give resilience. The stake is a small anvil, against the edge of which the gunflints are shaped and trimmed. Its height, termed the 'fall', varies with the type of gunflint. The block slopes toward the knapper, who, resting his elbow in the groin, grasps a flake, and with the thumb and forefinger of the left hand holds it on the stake, bulb of percussion downward. With his right hand the knapper taps the flake to get the hammer square. While the arm remains motionless, his hand moving from the wrist swiftly and flexibly taps the flake, which breaks crisply into as many as four sharp-edged squares if the flint is good. The flake is struck just inside the stake. The motion of the hand is kept up continuously on the leather stage while the knapper picks up a fresh flake. Three edges of the gunflint are trimmed, as the flake is turned in an anti-clockwise direction and the fourth is completed after the flint is struck from the flake. This trimming of the gunflint is a distinctive feature. Between 1838 and 1848 French gunflints with their 'gnawed' heels were sent to Brandon to be trimmed at a cost of one penny each. As quickly as he makes them the knapper disposes of his products. With unerring precision he casts each gunflint according to size into one of the heterogeneous collection of tins, pails and buckets around him. It is remarkable that a knapper blindfolded can detect a gunflint of his own workmanship.

Before selling his products the knapper must count them. The tins are emptied and the flints laid out on a table. Five flints are counted at a time by drawing 3 with the right hand and 2 with the left, and each group thus numbered is termed a 'cast'. Five hundred a minute is an average rate while an expert can in this way enumerate 20,000 an hour.

A study of the numbers employed in the flint industry at Brandon is of some interest as a register of the vicissitudes of war and peace and the march of invention during the last century. The industry was the mainstay of the working classes of Brandon during the first three decades of the 19th century. During the Napoleonic War it is said that 200 were employed. Shortly after 1832 the percussion cap superseded the gunflint and the friction match supplanted the tinder-box. That this double progress paralysed the industry can be seen from the decline in the number employed by 1837, then computed by Mitchell at 70 or 80. This depression and the sale of the last consignment of gunflints to the British Government in 1838<sup>33</sup> did not damp the enthusiasm

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<sup>33</sup> Not 1848, as Rogerson, p. 531.



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of the munition makers who in the same year<sup>34</sup> induced 70 or 80 persons to purchase £25 shares in a Brandon Gunflint Company. By 1846 trade seems to have revived sufficiently to employ 100 men, but the revival was only temporary for in 1848 the company appears to have failed, perhaps because of the rivalry of the East India Company which had a factory in the town. Just before the Crimea War the last large scale order was executed—a matter of 11,000,000 carbine flints to Turkey, our ally. At this time about 10 men were employed in each workshop.<sup>34</sup> From the total in 1868 of 36 knappers in regular employment besides the 20 miners at Lingheath, there was a decline and then a revival which a decade later stabilised the numbers between 30 and 40. This lasted till the South African War when a brisk trade sprang up in tinder-boxes,<sup>35</sup> for the climate during certain seasons of the year rendered matches almost useless. The cessation of armed conflict was indicated by the reduction of the numbers in 1907 to 17 knappers and 5 diggers only, 3 of whom were in permanent employment. The Great War dealt a death blow to the industry. The knappers not on active service at first achieved success by marketing 'Allies' patent lighters at fourpence each until the imposition of a duty. By 1924 only 7 knappers remained and now but a shrunken remnant persists—1 in continuous and 2 in intermittent employment, while a septuagenarian miner still excavates in the style of his forefathers.

It has been said that the decay of the industry was due not to the falling off in demand but to an increased distaste for engaging in the trade. The knappers' shop was regarded as too confined and agriculture was preferred. This is only partially true. The knappers have foreseen the inevitable decline of their handicraft and have adopted more lucrative means of livelihood, but the primary impetus has come from dwindling orders. Skertchly scouted the legend of the knappers dying before the age of 40 from consumption caused by the inhalation of flint particles. Although some seem immune from its ill-effects the facts do not corroborate Skertchly's statement. Of eight men in one workshop seven died in early manhood while one father and three sons all died from the same complaint in four years.

In view of the extravagant claims advanced by Skertchly a note on the technical vocabulary of the miners and knappers may not be out of

<sup>34</sup> In 1858 one wholesaler and 7 flint-makers controlled the industry. In 1868 three masters employed 36 employees.

<sup>35</sup> Tinder-boxes contained a trimmed flint, a small bar of steel and a woven fuse neatly packed in metal box. Each box was as serviceable as its bulk in matches.



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place. Apart from the expressions already mentioned the following are unusual. In mining 'fleet' denotes proximity to the surface in a vertical sense, while 'gain' indicates the same horizontally. Decomposed chalk is termed 'dead lime', 'paps' or 'horns' are protuberances of flint, and 'hobbly' is an adjective with similar connotation. 'Gulls' are the large blocks of flint rarely found under the floor-stone. Natural hollows are 'gulper', 'potholes' in the chalk are dubbed 'shitholes'. Flint coloured like the rock of Gibraltar is 'gib', doubtless coined by some digger-soldier. Where pillars are left to support the roof the epithet 'jarm (jamb)' is applied. In knapping twisted flakes are called 'wrung', while a flake which does not 'run' is 'dub' and 'bruckly' if it will not flake easily. Meal-times are 'dockey', also used in the Fenland. The 'tellers' are those employed for counting the gunflints. The persistence of French terms in the book-keeping of the industry is remarkable. Gunflints are quoted 'per mille', while the phrases 'pierres a feu' and 'fusee flints' are current. Most of the knappers' vocabulary, it need hardly be added, is local dialect.

Walter Johnson records the use by the knappers of a system of conventionalized symbols in book-keeping. Although this system is unknown to the present generation of knappers, products of universal education, there seems good authority for its use among some of the illiterate knappers till the late 19th century. The only numeral employed in the notation is 7, because it is shaped like a pick it is alleged. All other numerals are combinations of the symbols x, 1 and -. Examples of this obsolete method are given below.<sup>26</sup>

The output of the gunflint industry has naturally varied with the fluctuating conditions of peace and war but the output per knapper per unit of time has altered little during the period for which statistics exist. A knapper of average skill can produce from 5,000 to 7,000 flakes in a day, while an expert has struck as many as 10,000 in the same time. Flints can be knapped at the rate of 300 per hour, though slightly more have been completed by knappers of outstanding proficiency.<sup>27</sup> This daily total of 2,500 gunflints is exclusive of the flakes used as raw material. As the same knapper produces both, due allowance must be

<sup>26</sup> 1 after x = 100; — =  $\frac{1}{2}$  of any number preceding it; XII — = 1250; XX7— = 2750; IIII— = 450; III = 5; XHHHIII 7HH = 1975; 0 = £; ① = 10/-; 1 = 1d; — =  $\frac{1}{2}$ d; x = 10; 1 = 1/-; jag = ∩ (W. Johnson) ∩ (W. G. Clarke); C =  $\frac{1}{2}$  jag.

<sup>27</sup> The varying numbers given in earlier accounts are due to differences in the length of the working day.



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made in calculating the total putput. One flake of average size is converted into two large and one small gunflints. In 1868 the total weekly output was 200,000-250,000 gunflints. Even in the relatively prosperous period from 1880 onwards the totals had sunk to 80,000 for the same time. By 1913 this had been reduced to one quarter, though occasionally those exported reached the level of 40,000 per week. At present, the maximum is 23,000 per week, a figure far remote from those of the early 19th century when from one workshop alone no less than 10 tons of gunflints per week were exported.

The prices of gunflints vary with the size and the quality of the flint and with the demand for this munition of war. During the Napoleonic War, the price per thousand rose to £2 2s, with the consequence that the knappers are reputed to have played pitch and toss with guinea pieces. The depression in the thirties cut the price to 8s and the general decline reduced this by half—a level at which it rested for half a century. At the same period the raw material cost 6s 6d a jag if it was floor-stone; an additional 1s 8d was paid for groundage and 1s for cartage. 12,000 gunflints could be made from one jag of excellent stone and 6,000 from ordinary quality.

The nomenclature of a gunflint can be seen in FIG. 2. The classification depends on four factors—size, quality of the flint, use of double or single-backed flakes, and the existence of a single or double edge, in the latter case there is no heel. The standard sizes are termed musket, carbine, horse-pistol, large gun (double-barrel), small gun (single-barrel), pocket pistol.<sup>38</sup> Other sizes and appellations have had a transient currency. The quality of the flint is denoted by the adjective, 'best' or 'superfine', 'seconds' or 'common'. The latter term covers a variety of colours—such as chalk-heel, mixed grey or spotted. 'Seconds' are thinner than 'bests', but of inferior workmanship or colour. 'Seconds' and 'commons' are single-backed. Double-edged gunflints are longer than single-edged of the same size. Double-backed are smaller than single-backed. It is obvious that an extremely wide range of products was possible. The variety of tinder flints was more limited. A gunflint of musket size can be utilized as a strike-a-light, the former contrasted with the latter having transverse in place of longitudinal ribs.

Large stocks of gunflints have to be kept as formerly orders came for as many as 2,000,000. When counted, the flints are packed in

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<sup>38</sup> 'Large swan' and 'Long Dane' are among the uncommon sizes.



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bags, and then in barrels or kegs. At one time gunflints were sold in the market place at Brandon, but the knappers preferred to cut out the buyers and sell direct to the merchants. The flints were then exported from either London or Liverpool. In 1870 the chief markets were South and North America, the Cape of Good Hope and the African Coast generally, New Zealand and Spain. Russia and Turkey had been exploited till recently. In 1884 Africa was undoubtedly the most extensive market, absorbing four million per annum. In 1907-8 Austria, China, Tibet and Oceania appear among importers, while sales were pushed in Java, Sumatra, Borneo and the Malay Archipelago.

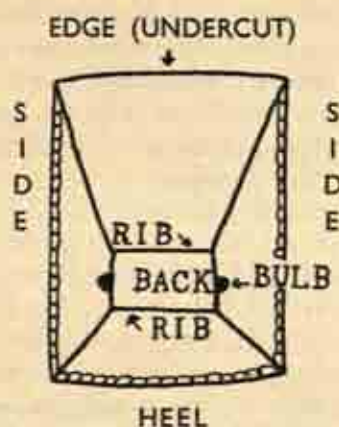


FIG. 2. GUNFLINT NOMENCLATURE: MUSKET SIZE

By the end of the Great War, West Africa and the Malay Peninsula area alone remained. At present, pistol gunflints (no. 2 size) are sent to Lagos, Nigeria; carbine (no. 4) to Kumassi, Ashanti and Calabar; and musket (no. 5) to Bangkok, Siam where they are probably used as tinder-flints.

In 1907 strike-a-lights were sold in South America, Spain and Italy, and to tropical travellers. The price was one penny each. Tinder-flints are no longer manufactured.

The by-products and waste-products of both the mining and knapping industries are of no little interest, especially to the archaeologist. Beside their major occupation of manufacturing gunflints for the victims of unscrupulous gun-running the knappers still produce squared

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flint-facings for churches and domestic structures. The cores resulting from flaking are squared or rounded up and used for building purposes. In 1876 these sold at 4s per 100. A very considerable trade in building flints existed at Brandon about 1904. Formerly the chalk blocks from Lingheath were used for rough walling. The waste products from flint-knapping consist of flakes of irregular shape or of outer crust besides the chips resulting from the knapping process proper. The immense heaps which in 1818 'would astonish a stranger on account of their magnitude' were still to be seen in 1876, although thousands of tons had been used on the local railways or had been dumped on Lingheath to fill up the mine shafts. Some of this waste was for a time utilized in pottery calcining and in the manufacture of china. Garden paths and roads assisted in the disposal of this cumbersome debris, which in bulk was about equal to the raw material. It has been estimated that the finished gunflints absorb about 13 per cent. of the total, building stone about 34 per cent., and the remainder is waste. Today the waste cannot be disposed of.

But it is for the fabrication of implements of flint that the knappers have earned notoriety in the eyes of prehistorians, for though arrow-heads, axes and scrapers of Brandon make rarely deceive an expert they are calculated to entrap the unwary amateur. Besides the production of copies for museums, the knappers also make ground and polished axes from cement, chalk and burnt umber coated with soda and then with gum shellac. The prehistorian owes a deep debt of gratitude to the knappers for initiation into the mysteries of flint working.

Among the many *tours-de-force* of the most cunning flint workers may be mentioned the production of fish-hooks with which perch have been caught in the Little Ouse; the fashioning of the alphabet in flint, and the creation of a necklace of flint bangles struck from solid discs of flint with infinite labour and patience.

Such is the Brandon flint-knapping industry of today. What of the morrow? We fear that the manufacture of these munitions of war can only look forward to a peaceful death in the near future. With the more general use of breech-loaders and the perfection of cartridges for the tropics, the demand for gunflints will cease and with that the supply. This unique domestic industry is already languishing into decay. Soon it will perish and be numbered among the bygone handicrafts of rural England, leaving only the tradition of the age-long secrets.



## Etruscan Tombs\*

by D. RANDALL-MACIVER

SINCE the great International Conference held at Florence in 1928, when so many of the wider problems of Etruscan archaeology were discussed, there has been a general tendency, especially marked among the younger writers, to concentrate upon intensive studies of detail. The seven volumes of the admirable *Studi Etruschi* and, in a less degree, the annual publications of the foreign Schools and the Italian Academy are full of excellent monographs upon such matters as the sculpture, painting or pottery of a given period or place, or upon any other minor department of the whole far-reaching subject. These are of real value as contributing to build up the entire fabric, but they are naturally too specialized to have any appeal for the general archaeologist.

The casual reader might suppose at first from its very modest title that Akerstrom's *Studien* amounted to no more than another of these minor works of limited scope. This would be a total misconception. It is a work of wide range and one of the most important of recent contributions to the systematic arrangement and chronology of Etruscan archaeology.

The development of tomb-structure is indeed traced from its earliest beginnings down to the 1st century B.C. on not less than 20 sites scattered all over Etruria. But *pari passu* with the architecture we are given a brief but masterly critique of the extant theories for each individual place, which results in a comprehensive review of the whole archaeological history. It would be impossible to praise too highly the industry, acuteness, and critical judgment of the author. He is profoundly well read and he has also studied the actual ground with an attention which lets nothing escape him. In his writing he has included the results of all the most recent research in every quarter.

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\* *Studien über die Etruskischen Gräber.* By Åke Akerstrom. Uppsala, 1934. Pp. 203, with 18 plates and 25 figures.

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The result is seen in a book, which, though not large in bulk, must be regarded as an indispensable supplement to all the more general works written a few years ago.

The difficulties of undertaking a systematic account of this kind can only be fully appreciated by those who have attempted a similar adventure. There are vast lacunae in the material; there are also labyrinths filled with rubbish. After more than 20 years we are still kept waiting for the publication of Veii. Though the excavations of Caere have been remorselessly carried on for ten years or more, the material from them, which might well fill two volumes as large as Dr Woolley's monumental books on Ur, has never been described except in a brief semi-popular article suitable to a shilling magazine. It is therefore inevitable and in no way a discredit to our author if his chapters on Veii and Caere appear to be sketchy, and are quite incommensurate with the importance of the subject. It is even remarkable that they do after all succeed in establishing one or two points.

It is with the third chapter, on Tarquinia, that he begins to show his mettle. Out of such paltry stuff as the letters and maddening résumés in the early *Annali* and *Bulletins*, the chit-chat of the egregious Mrs Gray and the haphazard remarks of early travellers, eked out by verbal traditions preserved by the workmen or the reminiscences of a disastrous Syndic, he has miraculously put together a complete mosaic. Tarquinia becomes a new thing, suddenly complete in its main lines even if the minor details are lost beyond recovery. Incidentally the location of the ancient city is successfully established. It was situated, not, as most writers have supposed, on the site of the present town of Corneto but on the Piano della Regina.

The treatment of Vulci is good if somewhat brief. Here one could have wished for more detail, but doubtless the author has used all that was relevant to his immediate purpose. Possibly he was a little diffident of encroaching upon a subject pre-empted by Messerschmidt.<sup>1</sup> It would be an excellent thing if one or other of these exceptionally acute researchers would put together a really complete and comprehensive account of everything to do with Vulci, omitting no part of its almost uniformly disgraceful history, in which Gsell's work marks the only bright moment. It might even be possible to make the Campana collection something more useful than parlour decoration. A commentary on Canina's entire *Etruria Marittima* would be most useful. In

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<sup>1</sup> e.g. in *Jahrbuch des Instituts, Ergänzung*, heft 12.



## ETRUSCAN TOMBS

the present chapter I note the wise caveat against accepting the Polledrara tomb in the British Museum as anything like a 'closed deposit'. It seems very doubtful whether it can be used at all as a dating point. Here is an opportunity for someone to come to its defence, if defence is possible.

The development of tomb-structure, to consider the subject which gives its title to the book, is to a great extent conditioned by the local geology. This standpoint, long maintained by Orsi, is well kept in view by Akerstrom, who devotes a chapter to an outline of the geology of the ancient province. In the volcanic section of southern Etruria, where the soft tufa can be carved as easily as Bath stone, it is natural that subterranean chambers should be cut in the rock whenever a large space was required. This explains the comparatively early origin of the elaborate chamber-tombs of Caere and Tarquinia. They have evolved from the earlier form of a mere corridor roofed over, of which the Regolini-Galassi is a standard example. Such corridors were at the outset no more than extensions of simple *fosse* or trench-graves, covered, as our author shows, by small tumuli. The enlargement which produced the *camera* or chamber tomb was occasioned by the increase in bulky tomb-furniture when the Etruscans rapidly became wealthy.

Outside the volcanic area the history of tomb-construction is different. It was due to the refractory nature of the local stone at Vetulonia that architecture was retarded in that district, and ultimately took the form not of subterranean chambers but of large structures of masonry. These tholos tombs such as the Pietrera and the Diavolino are described with penetrating analysis. It seems impossible to connect them, as is so often and so plausibly suggested, with the tholos graves of Southern Crete. As for any borrowing of architectural ideas from Asia Minor in the 7th century our author very properly rejects the idea and gives his reasons briefly but in crushing form—which will probably not deter other writers from resuscitating a frequently exposed fallacy.

The rock-cemeteries of the Viterbo district, *viz.* Bieda, S. Giuliano, Norchia, Castel d'Asso as well as the neighbouring Sovana form a small and homogeneous class. They are discussed in special chapters with some illuminating remarks. It is to be noted that in no case do they go back earlier than 600 B.C. Their carved temple-façades and doors show much resemblance to similar buildings in Lycia and Paphlagonia, which is due in all probability to the fact that each borrowed from Greece. Let me add, on my own account, that the theory of an immigration of the Etruscans from Asia Minor is strong



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enough to dispense with the most unwelcome aid given by those who mishandle the architectural evidence.

In dealing with the sites of Vetulonia, Populonia, and Marsiliana, the author has advantages which were not available in the case of Tarquinia. He is no longer obliged to scavenge for his material in the dust-heaps of an unscientific generation. These three places have been carefully and scientifically excavated, the results admirably published, and the whole subject discussed and coordinated by several modern writers. Here then we are at once enabled to recognize that Akerstrom is no less competent in criticism than in the reconstruction of evidence. He is careful, temperate and sound, neither revolutionary nor ultra-conservative. It is therefore a pleasure to recognize that he has rightly corrected me in an important point of chronology. He disagrees, very courteously, with my dating of the earliest appearance of oriental imports at Vetulonia, and would consequently bring down the date at which we can detect the occurrence of the first Etruscans on that site by a full half-century. I have long reflected upon this point and have come to the conclusion that he is almost certainly right. The date should be stated as 750 B.C. rather than 800 B.C. I am not, however, prepared to go one step further, as he suggests, and lower it to 725 B.C.

The beginnings of Tarquinia he puts, correctly in so far as the evidence allows of judgment, a generation earlier than that of Etruscan Vetulonia. On his argument therefore Etruscan Tarquinia begins about 750 B.C. Whether this can be accepted as an absolute dating depends upon the view taken of that most difficult question the 'Warrior's Tomb'. The highest dating for that tomb which now finds any general acceptance is 800-750 B.C. The reasons for preferring 750 B.C. are here carefully set forth, but we must proceed cautiously and for the present I would say no more than 'sub judice lis est'.

Much of the argument for chronology in this book depends upon the debated date of the foundation of Cumae. Not that Akerstrom falls into the common error of supposing Cumae to have been the sole or even the principal channel of oriental commerce; he is too clear-sighted for that. But as we know the *relative* chronology of Cumaean pottery very well, and this pottery is synchronous with oriental imports, the establishment of an absolute date is very important. He decides for 750 B.C., virtually the same decision that I made some years ago when I argued for 740 B.C. I believe this to be almost exact and cannot haggle about 10 years. The pre-Cumaean geometric pottery found in



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Etruria he believes virtually to overlap with the earliest Cumaeon. On this matter the evidence must be very carefully scrutinized before any decision can be made. Before leaving this subject however it must be said that Åkerstrom's account of the whole, vitally important, question of the geometric pottery in Etruria is the clearest and most consistent that has been given by any writer.

There can be no doubt that this book irons out many disagreements among other authors, and comes very near establishing a chronological scheme which should be acceptable to all workers in the same field. If there still remains a difference of plus and minus 15 or 20 years at some points, yet this is no more than just that margin of prudence which is desirable in order to allow for reasonable and legitimate differences in interpreting identical material. In many cases even this margin begins to disappear and we are approaching a very fine stage of definition.

It may be pointed out that the publication of this volume is exceedingly timely and appropriate in view of the approach of the second International Congress on Etruscan subjects which it is proposed to hold in Italy in the summer of 1935.

## Agriculture and the Flint Sickle in Palestine\*

by E. CECIL CURWEN

IT does not seem to have been contested, and therefore one may assume that it is generally agreed, that the discovery of the remains of a sickle in an ancient deposit is evidence that its owner knew how to grow corn. The forms that sickles took prior to the use of metal have been ably classified by M. Vayson de Pradenne in an article which was reviewed and summarized in *ANTIQUITY* in June 1930, and as a result of his work the characteristics of the flint flakes and of their method of hafting as sickles has come to be widely known and easily recognized.

The first part of M. Neuville's paper discusses the significance of the recent discovery by Miss Dorothy Garrod of flint sickles in a stratified mesolithic context in a cave in Mount Carmel.<sup>1</sup> Her sickle-flakes, with beautifully carved bone handles, occurred as early as the lowest Natufian<sup>2</sup> levels, which our author is disposed to assign to a considerably earlier date than the evidence of agriculture found elsewhere. The problem is—how does this fit in with the prevalent modern view, regarding Europe at least, viz., that the knowledge of agriculture is one of the factors which distinguish the neolithic from the mesolithic or epipalaeolithic? The natural answer, and the one which the author favours, is that, granted the correctness of the modern view, yet agriculture must have been first discovered in some one locality by people who were as yet living in an epipalaeolithic state of culture. What Miss Garrod's sickles show, as M. Neuville points out, is that this momentous discovery is likely to have taken place in Palestine, and that the knowledge of corn-growing matured locally among an epipalaeolithic people for a very long time before it reached that stage where

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\* *Les débuts de l'agriculture et la faucille préhistorique en Palestine*. By René Neuville. Extrait du Recueil de la Société Hébraïque d'Exploration et d'Archéologie Palestiniennes. Jerusalem, 1934.

<sup>1</sup> 'The Stone Age of Palestine', *ANTIQUITY*, June 1934, p. 133.

<sup>2</sup> The place to which the Natufian and other periods have been assigned in the absolute chronology of the Middle East will be found by referring to the Chronological Table published in *ANTIQUITY*, June 1932, vi, 185.



## AGRICULTURE AND THE FLINT SICKLE IN PALESTINE

it could exert the stabilizing effect which led to the first rise of the most ancient civilizations. It thus preceded by a considerable time all evidence of the other arts of civilization, such as the domestication of animals, the making of pottery and the working of metals. The distinction between epipalaeolithic and neolithic, while useful in Europe, is thus not really applicable in the same way in those lands which formed the cradle of civilization.

The attribution of the scene of the birth of agriculture to Palestine is fully in accord with tradition and with botanical evidence, as pointed out by the present reviewer in *ANTIQUITY*, 1927, 1, 262.

The second part of the paper deals with the typology of flint sickles in Palestine found in stratified deposits ranging from Natufian 1 to the end of the Bronze Age, when they were ousted by sickles of iron. First of all the author discusses the question of the brilliant gloss that is found on a large proportion of sickle-flints: can it be taken as evidence as to whether a given flint blade has been used as a knife, a saw or a sickle? He argues that the gloss merely indicates hard usage, the result of friction, and criticizes the present reviewer's conclusions, based on experiment and published in *ANTIQUITY*, 1930, IV, 184-6, that such lustre is only produced by substances containing silica in their composition, such as straw and wood. Our contention was that sickles could be distinguished from saws by the character of the lustre produced, straw giving a diffuse gloss, and wood merely a narrow band along the cutting edge, because a flint saw cannot bite deeply into wood by reason of its v-section. Bone, moreover, we argued, produces no lustre on flint, because it contains no silica. M. Neuville's objection is partly justifiable because he found, in repeating our experiments, that the degree of lustre produced by two hours work cutting straw could be washed off with water and a brush, and therefore consisted merely of a superficial deposit and was not a true lustre.

But his objections are also partly based on a curious misconception. He assumes that we drew a distinction between lustre that is evenly distributed and that which is striated—an idea that never occurred to us and which certainly finds no place in the paper criticized. Apparently this misunderstanding arose from the fact that our photograph (No. 3 facing p. 182) of the experimental gloss resulting from cutting wood shows a striated appearance which is, however, merely due to the slightly uneven rippled surface of the flake—a fortuitous fact of no significance.

M. Neuville also cites for comparison the gloss sometimes found on the cutting edges of flint celts that have not been intentionally ground or



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polished—a gloss which exactly resembles that on the sickles but which has clearly resulted from friction in splitting wood. (This is, of course, because a flint axe will penetrate deeply into wood, whereas a flint saw will not). He argues from this that the apparent differences in the lustre on flint blades result only from the degree of use, the quality of the flint, and the fineness of the grain of the flint, and not from the nature or quality of the material cut. But axes are not really comparable with saws and sickles owing to the difference in their mode of use.

As a result of these criticisms we have had some fresh experiments carried out with the kind assistance of Mr D. W. Hudson. In order that the effects of prolonged friction might be determined on two different kinds of flint, cylinders of oak, bone and compressed straw (in the form of strawboard) were spun on an electric lathe, and the flint blades were applied with sufficient pressure to cut into each kind of substance. Two varieties of flint were used, both being in the form of recently struck flakes; one kind was black flint from Brandon, the other a coarse grey flint from Norwich. In each case the flakes had been given a serrated edge by the late Fred Snare of Brandon.

Two flakes, one black and one grey, were applied for 30 minutes to the drum of strawboard while the latter was revolving at 3500 revolutions per minute, so that at a rough calculation about 10 miles of compressed straw came into contact with the flints. The result in the case of the black flint was to produce a fairly bright lustre, most brilliant on and near the serrated edge, and gradually fading towards the opposite edge. The appearance exactly resembles that on the ancient sickle-flints, but is somewhat less brilliant; it cannot be removed by brush, water or a grease-solvent such as chloroform. The grey flint proved far less susceptible to polish, for the lustre affected little more than the serrations themselves, but these had their sharp edges thoroughly rounded and smoothed. How much standing corn corresponds to a given quantity of strawboard in regard to its polishing qualities is a ratio that would be hard to determine, but if it be provisionally estimated at 100 : 1, and if we assume that with so small a tool the corn was cut furrow by furrow, then 1000 miles of furrow is contained in nearly 100 acres of corn. It is at any rate obvious that the highly polished flint sickles must have seen very many seasons hard work.

The cylinders of oak and bone were each run for 30 minutes at 2500 revolutions per minute, during which time serrated flakes of black and grey flint were made to cut into them as deeply as possible. Thus in each case about 7 miles of wood and of bone came in contact





PLATE II



PREHISTORIC SICKLE FLINT FROM SELMESTON, SUSSEX, SHOWING GLOSS. x 24  
P/A. Max Mith



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with the flints. In the case of bone a certain amount of splintering of the serrations occurred, but not the slightest trace of polish of any kind, either on the black or the grey flint. With the oak, on the other hand, a narrow, well-defined band of rather dull but very definite lustre, rather less than an eighth of an inch wide, was produced along the cutting edge, more noticeable on the black than on the grey flint, but quite definite in both. Its circumscribed character is in marked contrast to the diffuse gloss produced by straw, and is due to the inability of a flint saw to penetrate deeply into a rigid medium like wood. A flint axe, on the other hand, will penetrate deeply into wood and therefore may receive a diffuse lustre resembling that produced by straw on a sickle-flint.

These experiments, though far from being exhaustive, do surely support the view that the polish on the flint blades is not merely the result of hard usage, but is conditioned by the nature of the material cut; that the conditioning factor in such material is in all probability the presence of silica; and that the diffusion of the gloss on the ancient flint blades indicates that this siliceous material must have been of a yielding character, unlike wood. As corn-straw is of this nature, and as such blades are known to have been used in sickles, it seems a safe inference that diffuse gloss on a flint blade is the result of cutting corn or other siliceous grasses.

M. Neuville's classification of flint sickles is based on the examination of very large numbers of examples from stratified deposits, and should prove useful. Points of special interest include the following:

(1) Bone mounts for the sickle-flints appear to be confined to Natufian I. These are finely shaped and carved, and the flints themselves are the most delicately formed in the whole series, the average ratio of length to breadth being  $4\frac{1}{2} : 1$ . Thereafter we may infer that the mounts were of wood as in the case of the known examples from Egypt.

(2) Coarse serrations first appear on the flints during the Tahunian, which is the phase which intervenes between the end of the mesolithic (Natufian IV) and the appearance of pottery, copper and polished flint in the Ghassulian. The Tahunian also produces the first flint celts.

(3) In Tahunian deposits there are also fairly numerous large flint blades, with lustre from use on one or both edges, and the author suggests that such may have been used as sickles. We would urge that if this lustre is diffuse such use may be regarded as certain.

(4) Fan-shaped scrapers, typical of the Ghassulian (the age of Sodom and Gomorrah), show similar lustre on their edges.



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(5) Coarse serrations become usual during the Bronze Age, but are not found in the Ghassulian.

(6) Flint sickles are the only flint work found in Palestine during the middle and late Bronze Ages (2000-1200 B.C.). It is perhaps because the art of skilful knapping was lost that the sickle-flints of this period become large and coarse, short and broad, the average ratio of length to breadth being  $1\frac{1}{2} : 1$ .

(7) The fine flint blades with corn-gloss on them (one-piece sickles?) are common only between the 19th and the 15th centuries B.C.

The author complains that excavators on Bronze Age sites pay less attention to the stratification of stone tools than those on prehistoric sites, with the result that it is not possible to determine the evolution of the flint sickle during the Bronze Age on the evidence at present available. This complaint is no doubt justified: the reviewer has met with a similar difficulty in England in regard to the development of querns.

The author is, however, surely wrong in saying that the flint sickle disappeared suddenly on the arrival of the Israelites in Canaan. The date of the conquest by Joshua is now established at about 1400 B.C., whereas it has already been stated that flint sickles continued in use till about 1200 B.C. It was the arrival of the Philistines, bringing iron, that caused this ancient and tenacious form of implement finally to disappear.

To sum up: this is an excellent paper—cautious, moderate and thoughtful; and being the work of no arm-chair critic but of an experienced field-worker, is well worthy of our confidence.

### ADDENDUM and PLATE II

By way of comparison with the flint sickles of Palestine it may be worth while publishing here the photograph<sup>3</sup> of a specimen of an extremely rare type of flint sickle from Britain. It has been recently found in Sussex in circumstances which seem to favour a possible neolithic or ('Peterborough') connexion; the writer has only seen two other specimens of similar type from the rest of Britain. Their use as sickle-flints is betrayed by the brilliant diffuse gloss which is found on both faces adjacent to the cutting edge.<sup>4</sup>

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<sup>3</sup> I am greatly indebted to the Editor for having the photograph specially taken.

<sup>4</sup> The specimen from Selmeaton was fully described in the *Antiq. Journal*, xiv. Another example, from Salvington, will, it is hoped, be published shortly, and is in the collection of its finder, Mr Barclay Wills, of Worthing.



# The Ancient Maya Causeways of Yucatan

by MARSHALL H. SAVILLE

*American Museum of Natural History, New York*

THERE is little room for doubt that the remains of ancient roads or causeways still to be seen in Yucatan are, so far as the history of the peninsula is concerned, of considerable antiquity.\* Two of the earliest and most important historians of Yucatan, namely, Diego de Landa and Bernardo de Lizana, mention them, and brief notices of their existence have been made by explorers in the nineteenth century. It seems highly probable that as early as a thousand years ago a broad highway extended a considerable distance across the northern part of the peninsula of Yucatan, if indeed it did not reach from the eastern to the western shores of the country.

When the Spaniards first entered this part of Middle America during the first half of the sixteenth century, they found the country in a state of decadence. They established their colonial capital, to which they gave the name of Merida, on the site of an ancient and important native city called Tiho. To the east of Merida, the town of Izamal was founded on the site of a very important and holy Mayan city known as Ytzamal.

Bishop Landa, writing during the last quarter of the sixteenth century, described the multitude of edifices in Yucatan, noting the extensive groups of Tiho, Ytzamal, and Chichen Itza. Of Ytzamal he noted that 'there is no memory of their builders, who seem to have been the first inhabitants of the land'. He stated also that Tiho and Ytzamal were about thirteen leagues apart, and said 'there are signs even today that there was once a very handsome causeway from one city Tiho to the other Ytzamal'. He further wrote that Tiho belonged to a period as ancient as Izamal. From Bishop Landa's statement we know that the causeway was in a ruined condition 350 years ago. In 1890 the writer saw numerous traces of this road when journeying from Merida to Izamal.

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\* They are called in the Maya language, *sacbe* or *sacbeob*, plural.

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Madame Le Plongeon wrote that they saw between the village of Mucuiche and Izamal, on the left of the road, 'the remnants of the magnificent ancient causeway, carefully built of hewn stones, cemented with mortar, which, at the time of the Spanish conquest, existed between Izamal and T.-Hó (Merida). A great part of this work has been thoroughly destroyed to obtain stones to macadamize the public road'.

A few years after de Landa wrote his history (not known or published until 1864), Lizana, in describing the 'idols which were venerated in the town of Ytzamal when the land was conquered', wrote concerning the importance of the town in the religious life of the Maya as a place of pilgrimage, saying: 'there they offered great alms and made pilgrimages from all parts of the province, for which reason there had been made four roads or causeways to the four cardinal points, which reached to Tabasco, Guatemala, and Chiapas, so that today in many parts may be seen pieces and vestiges of it. So great was the concourse of people who assisted at the ceremonies to these oracles of Ytzamat-ul and Tiab-ul, that they had made these roads'. Here again we have the statement that the roads were in ruin a short time after the Spanish conquest and trustworthy information concerning their vast extent.

Shorter interurban causeways are found in two sites in Yucatan. De Landa mentions a beautiful causeway leading from the patio of two small edifices on pyramids to the so-called Sacred Well or Cenote of Sacrifice at Chichen Itza. In 1891 my interest in ancient roads was aroused during the months I was engaged in excavation at the ruins of Labna, for I had to cross daily an ancient causeway extending 600 feet from the Palace to the Temple. This road was in an advanced state of ruin, and was not more than 25 feet wide and about 4 feet in height.

During the past years attention has been drawn to the hitherto unexplored city of Cobá, which lies to the eastward of Chichen Itza some seventy miles. From what we now know about Cobá it appears that this place was one of the largest and most important of ancient Maya. In 1842 the famous explorer Stephens, when at the frontier town of Chemax, on the road from Chichen Itza to Cobá, was shown by the cura of Chemax a report which he had drawn up relating to his curacy of Chemax, which included all the territory to the eastward as far as the sea. Stephens copied and translated the portion concerning the ruins of Cobá and the causeway leading from it. In this report is described a large two-story edifice called the Monjas, followed



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by the statement : ' from this edifice there is a *calzada*, or paved road, of ten or twelve yards in width, running to the southeast to a limit that has not yet been discovered with certainty, but some aver that it goes in the direction of Chichen Itza '. Stephens adds : ' the most interesting part of this, in our eyes, was the *calzada*, or paved road, but the information from others in the village did not interest us. The cura himself had never visited these ruins ; they were all buried in the forest ; there was no ranch or other habitation near ; and as our time was necessarily to be much prolonged by the change we were obliged to make, we concluded that it would not be advisable to go and see them '.

In later time, Charnay, on his visit to Yucatan in 1880, wrote : ' we also found marks of a cemented road from Izabal to the sea facing the island of Cozumel ', and he places Cobá in approximately its correct position on his map, although he does not mention the site in his text. From a series of letters published in Paris while he was in Yucatan we learn that he planned on two different occasions to go to Cobá, but was deterred by the unsettled condition of the country resulting from the hostile activities of the Chan Santa Cruz Indians.

On the expiration of my work at Labna, April 1891, my friend Don Antonio Fajardo, in Ticul, urged me to undertake a trip to Chemax in order to investigate a great ruined city which he stated was near a large hacienda owned by him, some distance to the east of Chemax. In September of the same year Teobert Maler saw this ancient causeway running to Cobá ; he reached the ruins and made several photographs, but he kept his knowledge to himself, issuing no description of his visit. As Maler lived in Ticul and was on very friendly terms with Don Antonio, it is probable that he learned of the ruins from his neighbour. Hence we see that knowledge of the causeway and Cobá was not confined solely to the Indians. However, only in recent years has it been safe to go into this region, as the Indians of Chan Santa Cruz were in control of the country, and no extensive explorations could have been carried out.

It remained for Dr Thomas Gann to be the first to visit the site, under the auspices of the Carnegie Institution of Washington, in the winter of 1926. In the report on four expeditions to Cobá during that year by different investigators no mention is made of the causeway, but, in his book published during the autumn, Dr Gann speaks of it. He says that the road is 32 feet wide and varies from 2 to 8 feet in height. He first encountered it about 6½ leagues beyond Chemax and followed



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it about 4 leagues towards Cobá. He writes; 'the road represents an enormous expenditure of time and labour, involving the quarrying, transport, facing, and building in, of nearly a million tons of stone, and is unique throughout the whole of the Maya area, for though cement covered roads exist in and around many of the ruined cities, no such elevated causeway has been found elsewhere'. Again he writes: 'on each side of the road were great quarries from which the stone used in its construction had been taken. Holes were apparently sunk round the great blocks, in which they built fires, and then pouring water into the red-hot holes, cause the rocks to split, so that slabs of it could be easily dug out. The sides were built of great blocks of cut stone weighing hundreds of pounds; the central part was filled in with unhewn blocks of limestone, and the top covered with rubble, which, as is indicated by the traces of it which remain here and there, was once cemented over . . . It was convex, being higher in the centre than at either side, and ran so far as we followed it, straight as an arrow, and almost flat as a rule'.

A member of the third Carnegie Expedition to Cobá in 1926, was J. Eric Thompson, who has recently published a much fuller account of the causeway. From Thompson's account, published in Spanish, I quote as follows: 'noteworthy are the great number of roads which appear to radiate from the metropolis of Cobá, undoubtedly one of the most important of Maya cities, being only surpassed by Tikal in the extension and number of temples and mounds which it contains. Of these roads, we counted eight, and Indians of confidence who have travelled in the mountains told us of two or three more. They are of variable height, but the great road seen by Dr Gann, which was discovered by the Austrian archaeologist Teobert Maler, has a width of approximately ten metres. Another road which united the city of Cobá with the sacred ward of Macanxoc has, however, a width of twenty metres more or less, but this is exceptional.

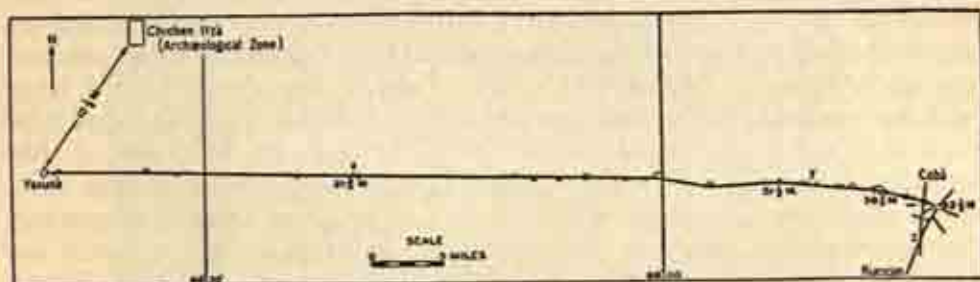
The great road called by Dr Gann the Camino Real de Occidente, does not, so far as we could determine, connect Cobá with the ruins of Chichen Itza, but it seems to end at Yaxhuana, a ruin whose architecture very much resembles that of Cobá. It is situated about ten miles south of Chichen Itza. The distance between these two cities is some sixty miles. On account of lack of time and scarcity of water, we were not able to go over the road more than a distance of ten or eleven miles, the road running in a straight line without any curves or deviation due east in the direction of Cobá. In the last half-mile which we



## THE ANCIENT MAYA CAUSEWAYS OF YUCATAN

traversed when we were already entering the wards of Cobá the road is divided into two sections: one goes towards the ward of the north called Nohoch Mul, and the other in an extended curve ended at the foot of the highest hill in the city proper of Cobá, situated on the isthmus between the two lagoons of Cobá and Macanxoc. In nearly all the entire road which we traversed it reached an elevation of a metre more or less, but when we came to depressions it maintained its proper level, undoubtedly to contend with the peril of inundation during the rainy season. So that in various places where there are depressions the road reaches a height of three to four metres.

The bed of the road consists of the typical mixture of stones such as are found in ruins of the ancient Mayas; that is to say, great unworked stones. Above this is a layer of smaller stones held together with a



Drawing to scale of the Yaxuná-Cobá causeway, the greatest of all the causeways of the ancient Maya thus far discovered. This road, 62½ miles long and from 30 to 34 feet wide, was built of stone and surfaced with cement. In March 1933 it was traversed from end to end for the first time in modern days by an expedition despatched by the Carnegie Institution of Washington. The road roller was found at X and the date-inscribed stones at Y. The intersecting roads, shown in the accompanying illustration, are at Z. Sixteen of these great roads centred at Cobá, not all of which are represented in the diagram.

*By courtesy of the Carnegie Institution, Washington*

mixture of lime and saccab, and over this is a typical pavement of plaster made of lime and saccab which appears to be almost a cement. Of course the floor has been almost completely destroyed. The sides of the road were made of walls of stones roughly squared, and of sufficiently regular size (PLATE I). It seems certain that these walls were formerly covered with plaster in ancient times, but today there remain no signs of it. These roads are much damaged by the great trees whose roots have thrust into the cement, tearing up the stones, and as the trees have fallen they brought up great masses of mixed stones cemented together. Without doubt, however, with only a little repairing these roads would serve well for automobiles.

There is another road which unites Cobá with Kucican, a ruin

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which we found some ten miles to the south, and for a number of miles has an elevation of six to seven metres. Near Kucican there are various passages made under the road, constructed with the typical Maya roof of the ancients. These tunnels would permit travellers to go from one side of the road to the other without having to climb over them. A short distance from Cobá this same road joins another which seems to come out from the sacred ward of Macanxoc. The roads connect at an angle of 35 to 40 degrees, and in the angle forming this junction is a small ruined building.

Through the interest aroused by the accounts of this partial exploration of the great Cobá causeway, Captain Robert R. Bennett was led to undertake an expedition to Yucatan to try and follow the road from its unknown western terminus through to Cobá. He set out during the winter of 1930 under the aegis of the Museum of the American Indian. The journey and publication of the results added no important information of this phase of the Maya civilization, representing, as it does, an engineering work of great magnitude. As a later explorer remarks, Bennett asserted that the expedition was successful in ascertaining the destination of the old causeway, but, 'in spite of this assertion he failed to attain his major objective, since he was able to traverse only a small section of this *sacbe*, and was obliged to assume that its western terminus was Yaxuna, as previously suggested by Thompson'. Bennett made a short visit to the ruins of Yaxuna. There he indicated, as the terminus of the *sacbe*, a very high mound with a crumbling temple. In fact the terminus is a small mound without any trace of superstructure.

The real exploration of the ruins of Cobá was accomplished by the sixth Carnegie Expedition, which arrived on the ground a few weeks after the visit of Captain Bennett. From the preliminary report published by the Institution, we learn that 16 *sacbeob* or causeways make the Cobá archaeological zone absolutely unique in ancient America. They range in length from 100 kilometres, to only a few hundred metres, in width from 19 metres to less than 4 metres, and in height from 5 or 6 metres to less than 1 metre.

The last, and completely successful exploration which was carried on throughout the entire length of the main road, was made in February and March 1933, by Alfonso Villa R. for the Carnegie Institution. The report, just issued, states that the causeway is exactly 100 kilometres long, maximum height 2.50 metres, average height 75 cm. and average width 9 to 10 metres. It runs in an absolute straight line from



PLATE I



ANCIENT CAUSEWAY OF THE MAYA: SIDE VIEW OF THE YASUNÁ-CORÁ ROAD, YUCATÁN  
*By courtesy of the Carnegie Institution, Washington*

PLATE II

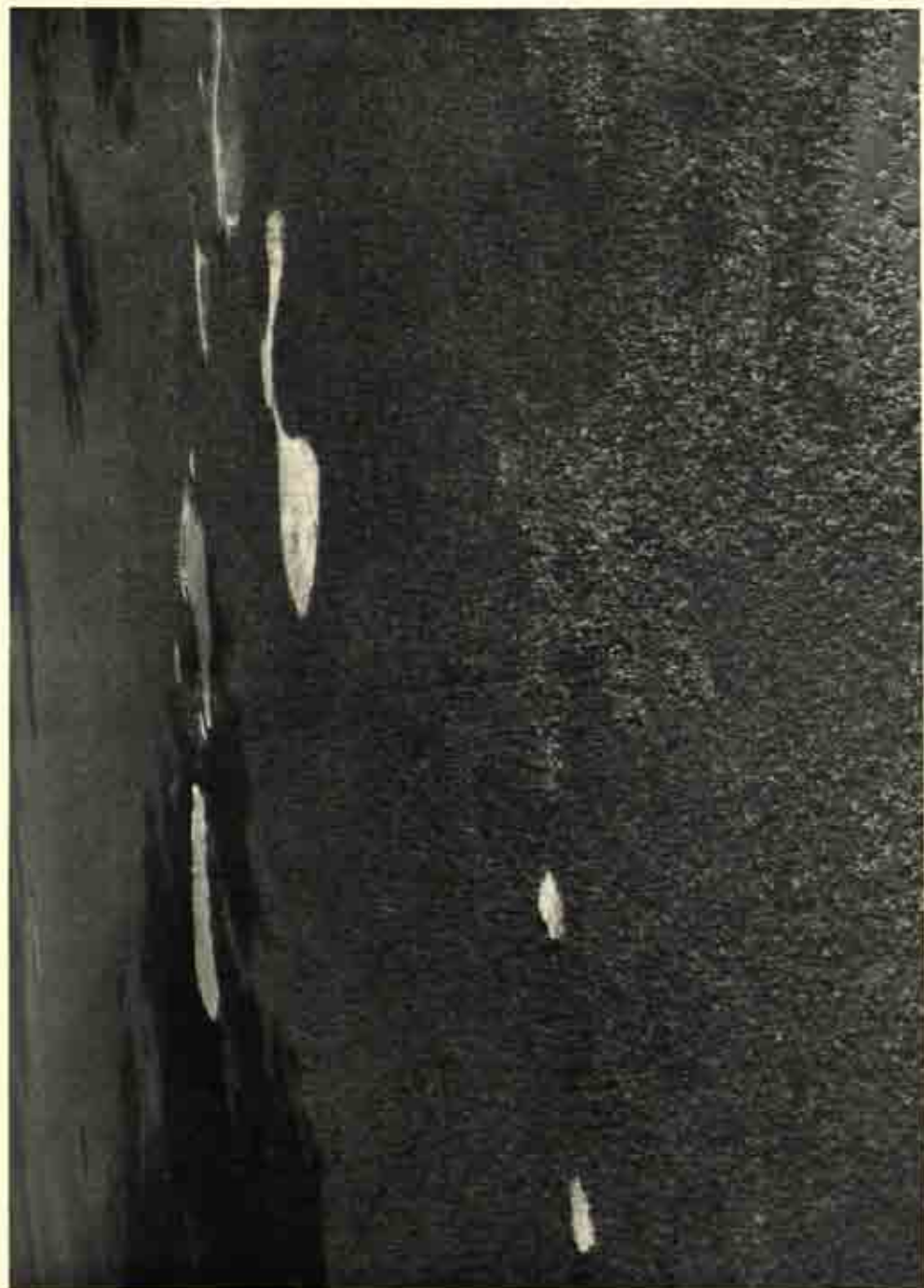


THE 'FIRST AMERICAN ROAD ROLLER', FOUND ABOUT 22 MILES OUT OF YANUNA ON THE ANCIENT  
MAYA CAUSEWAY LEADING TO CORA, YUCATAN

*By courtesy of the Carnegie Institution, Washington*



PLATE III



AIR-VIEW OF ANCIENT MAYA ROADS OR "SACBEOB" AT COHA, YUCATAN, SHOWING THE CHAIN OF LAKES  
*By courtesy of the University of Pennsylvania Museum, Philadelphia. Patrick Aerial Service Photograph*





## THE ANCIENT MAYA CAUSEWAYS OF YUCATAN

its western terminus for more than two-thirds of the distance. An accurate map was made of the long main road, but the other 15 causeways still remain to be surveyed. An interesting discovery was an immense stone cylinder on one section of the causeway, 4 metres long, 70 centimetres in diameter, and weighing approximately 5 tons. The explorer, rightly I think, assumes that it was used as a road roller for levelling the top layer of plaster which once covered the surface. (PLATE II).

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### NOTE BY THE EDITOR

The technical interest of the air-photograph (PLATE III) is considerable. The course of the raised road-causeways is revealed by marks or lines across the top of the tropical forest. These appear to be caused by the fact that the trees growing on the causeway are naturally taller than the others, and by thus out-topping them they cast a shadow and become visible from above. It is not (presumably) because they grow better on the causeway; the opposite result rather would be expected—that they should be stunted. If my explanation is correct, there is then an essential difference between this photograph and one of a crop-site in England, though at first glance they seem similar—and though the *results* are in fact similar. The same phenomenon was observed, and photographed, by Major Allen in the case of the Wychwood Grim's Ditch near Tomlin's Gate in Kiddington, Oxon. The Maya photographs would have shown the roads better if they had been taken looking into the sun.

## 'Fossil Tradition' in Stone Implements\*

by A. VAYSON DE PRADENNE

*Professor of Prehistory at École d'Anthropologie de Paris*

WHEN one speaks of tradition, that is to say, of the transmission of human knowledge from one generation to another, one implies that there are two methods by which that transmission is effected—the spoken and the written word; thus one draws a distinction between oral and written tradition.

Nowadays written tradition alone is of importance; but one recognizes that it has gradually replaced that oral tradition which alone existed, as one supposes, in prehistoric times. We wish here to call attention to yet another mode of transmission which certainly played a part, perhaps even an important part, in the Stone Ages, and disregard of which might in certain instances seriously confuse the study of prehistoric archaeology. We refer to the use and imitation by primitive peoples of the stone implements or weapons that had been previously manufactured by the earlier inhabitants of the same region. Thus the style, or some elements of it, may have passed from one period to another—and this logically constitutes tradition—but without involving any direct personal contact between the successive masters of the technique. That technique is transmitted *post mortem* through the agency of objects taken from the soil, which have thereby become the *fossil* remains of the vanished culture. And that is why we call it 'fossil tradition', making use of an analogy and contrasting it with the processes of oral and written tradition. Even the modern world, with its rare survivals of Stone Age cultures, provides instances of this practice. The natives of Guiana, who no longer make stone axes, collect (or used a few years ago to collect) the axes ground and polished by the pre-Columbian inhabitants, fitting them into their wooden-handled clubs. Examples may be seen in many ethnographical collections<sup>1</sup> (FIG. 1).

\* Translated by the Editor.

<sup>1</sup> We are well aware that objects of this kind have often been made in Europe by inserting authentic Brazilian polished stone axes into equally authentic Carib clubs. Monsieur Blin, the well-known collector of ethnographic specimens, used with his usual good-nature and skill to create many such unions for his colleagues. But the ethnographic fact is well established, and existed free from any possible European influence.



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Thus, both in the Pitt Rivers Museum at Oxford, directed by Mr Henry Balfour, and in the Horniman Museum in London, directed by Mr Harrison and arranged on similar lines, one can see ancient stone arrow-heads picked up by modern North American Indians who attribute certain magical powers to them, and who make (or made until

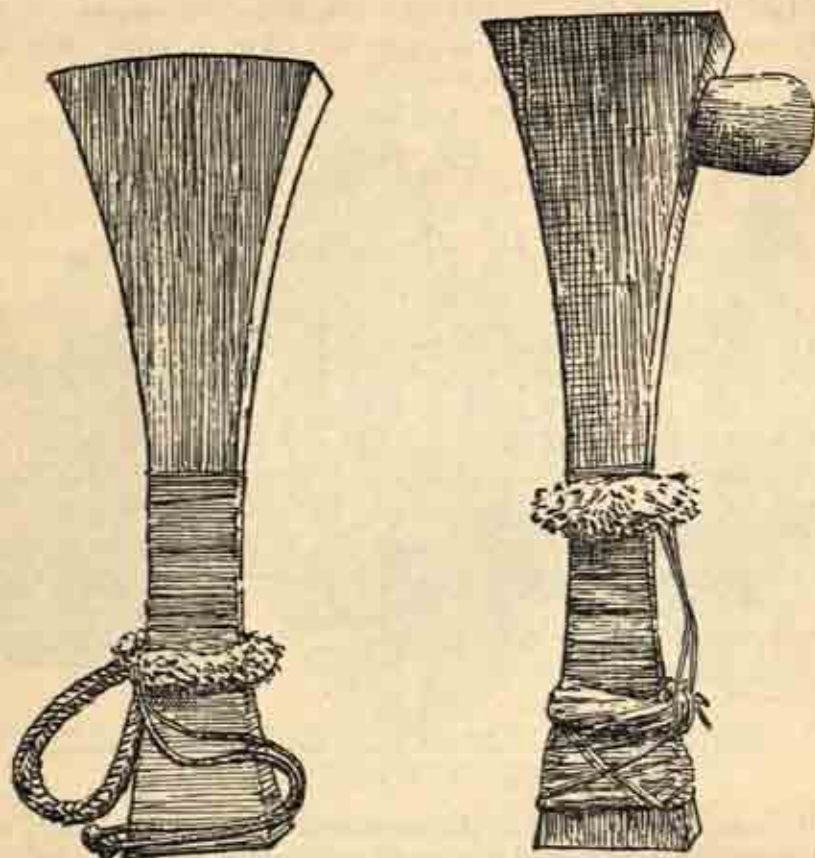


FIG. 1. CARIB CLUBS WITH RECTANGULAR SECTION:  
in one the native has inserted an old stone axe. British Guiana: in the British Museum

recently) similar ones for use as weapons. In this case, it is true, living tradition possessed the same technique of workmanship, and fossil tradition was merely added on the top of it, so to speak.

Excavators have for long past noticed and recorded the occurrence in prehistoric deposits of objects with two distinct patinas, that is to

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say, of objects that have been re-worked long after their first usage. Thus in the celebrated Mousterian deposit of La Quina (Charente), so successfully and scientifically excavated by Dr Henri Martin, there is a lower deposit specially marked by the deep yellowish patina of its flints. Now Dr Martin has obtained examples of these, easily identified by their patina, from amongst the objects found in the *upper* deposits. They had been re-used by the later inhabitants of the spot, who had lightly re-chipped them, to sharpen the edge or point, but without altering their form. The portions thus re-chipped had acquired the white patina characteristic of the later deposit (FIG. 2).

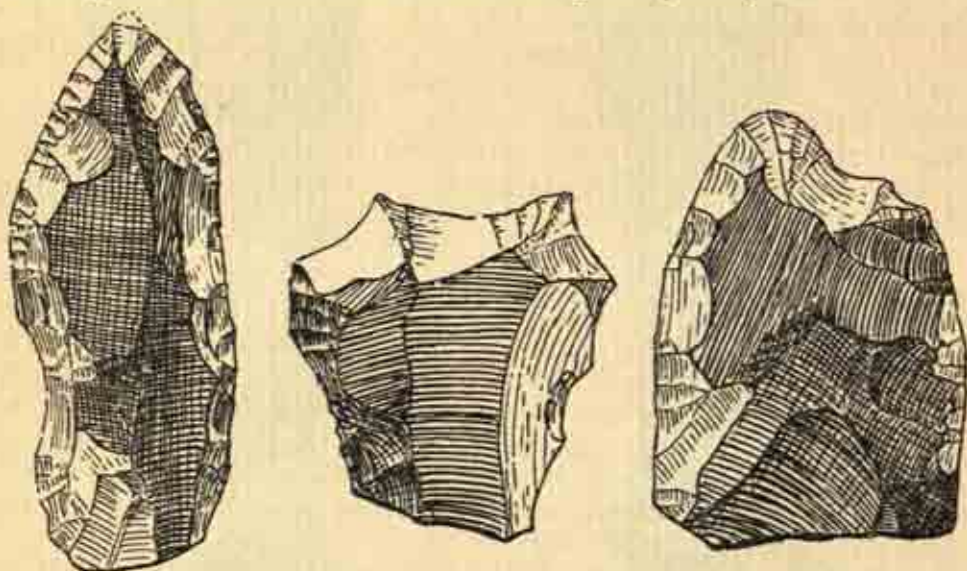


FIG. 2. LA QUINA (CHARENTE): Mousterian objects from the lower deposit with dark patina, re-worked during the period of the upper deposit (white patina). Excavations of Dr Henri Martin (1)

The same fact has been demonstrated at La Baumo-dei-Peyrards in Vaucluse by Deydier and Lazare, for objects also of Mousterian type. Implements from the lower deposit, with a white patina, have been found in the upper deposit, where the flint has kept its natural colour, having retouches dating from the second period.

It is true that at La Baumo-dei-Peyrards as at La Quina it is a question of the re-use of implements, after a long interval, by a people having still the same culture, retained doubtless by living tradition. We have here a parallel to that modern North American one cited above.



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In the quarries of Villejuif on the outskirts of Paris I found at the base of the *ergeron* or recent l $\ddot{o}$ ess a *biface* (hand-axe) of the Chellean or Acheulean period, marked not only by its shape but also by the deep lustrous reddish patina it had acquired from the quaternary gravels; and one of its ends had been re-chipped to a sharp point. The re-worked portion has the white patina characteristic of the deposit in which the implement was found, which corresponds with the Older Mousterian.



FIG. 3. VILLEJUIF (SEINE). Acheulean hand-axe from gravel (dark patina) re-used in the Mousterian period (white patina) ( $\frac{1}{3}$ )

Here is a clear instance of an object re-used in a period marked by a different culture (FIG. 3).

But now we have a much more important fact to face, in view both of the very considerable differences that exist between the culture of the borrowers and that of the model, and also of the confusion which it introduces into prehistoric studies.

One was aware of the differences of opinion which divided pre-historians on the matter of certain very peculiar specimens found in

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North Africa, and which consist of arrow-heads roughly chipped on one side only. Since 1883, when Frederic Moreau remarked on the association of one-sided (*uniface*) Mousterian chipping and the neolithic tang, the problem had attracted considerable attention from students of that area. De Morgan, Capitan, Boudy, attributed the objects to the Eneolithic period. Dr Gobert saw a peculiar development of the neolithic, on the lines of a Mousterian tradition. Pallary on the other hand regarded them as typical of a decadent industry which he called the 'Berber neolithic'. Since 1919 Reygasse had published the site of Bir-el-Ater (Wad Djebbana) where, under 8 metres of alluvial deposit of apparently pre-holocene age, was a huge hearth with numerous artifacts, whose general Mousterian character was supplemented by tanged specimens. For this industry he created the term Aterian, which today is well known. Quite recently in the same district M. le Dû has found a very similar site in Wad Djouf, under equally undisturbed conditions; the specimens are particularly fine and numerous. Further, there has been established the fact, overlooked by the earlier explorers, that there occur not only arrow-heads but also a whole group of implements—points, scrapers, bevels (*biseaux*), flakes, etc., supplied with handles (FIG. 4).

The Aterian industry, widely distributed throughout North Africa and extending right into the Sahara, is generally found on the surface or at a slight depth in brick-earths whose upper portion has been re-arranged. In the outskirts of Oran, over an area of many acres, there is a flint-site called Champ de Tir or Polygone d'Eckmuhl, where the objects are found both superficially and at a slight depth in the red clay covering the underlying limestone. This site was first published very summarily by Carrière.<sup>2</sup> M. Pallary has recently<sup>3</sup> supplied some more detailed information on this subject, and reproduced a series of tanged objects from it. He informs us that it was he who in 1885 made known to Carrière the Polygone d'Eckmuhl site. He regards it as 'berberesque'. Having in 1919 acquired the Carrière Collection, I had the first evidence obtained from Eckmuhl. Since then, under the excellent guidance of M. Pallary, I have visited the site and collected a certain number of specimens, to which M. Pallary has been good enough to add the results of some of his researches. The general character of the industry is curious and looks like a stunted example of the Aterian Industry. Its homogeneous character was obvious, because,

<sup>2</sup> *Bull. de la Soc. de Géogr. et d'Arch. de la prov. d'Oran*, 1886, pp. 147, 149.

<sup>3</sup> *Rev. Afr.* 1922, pp. 412-16.



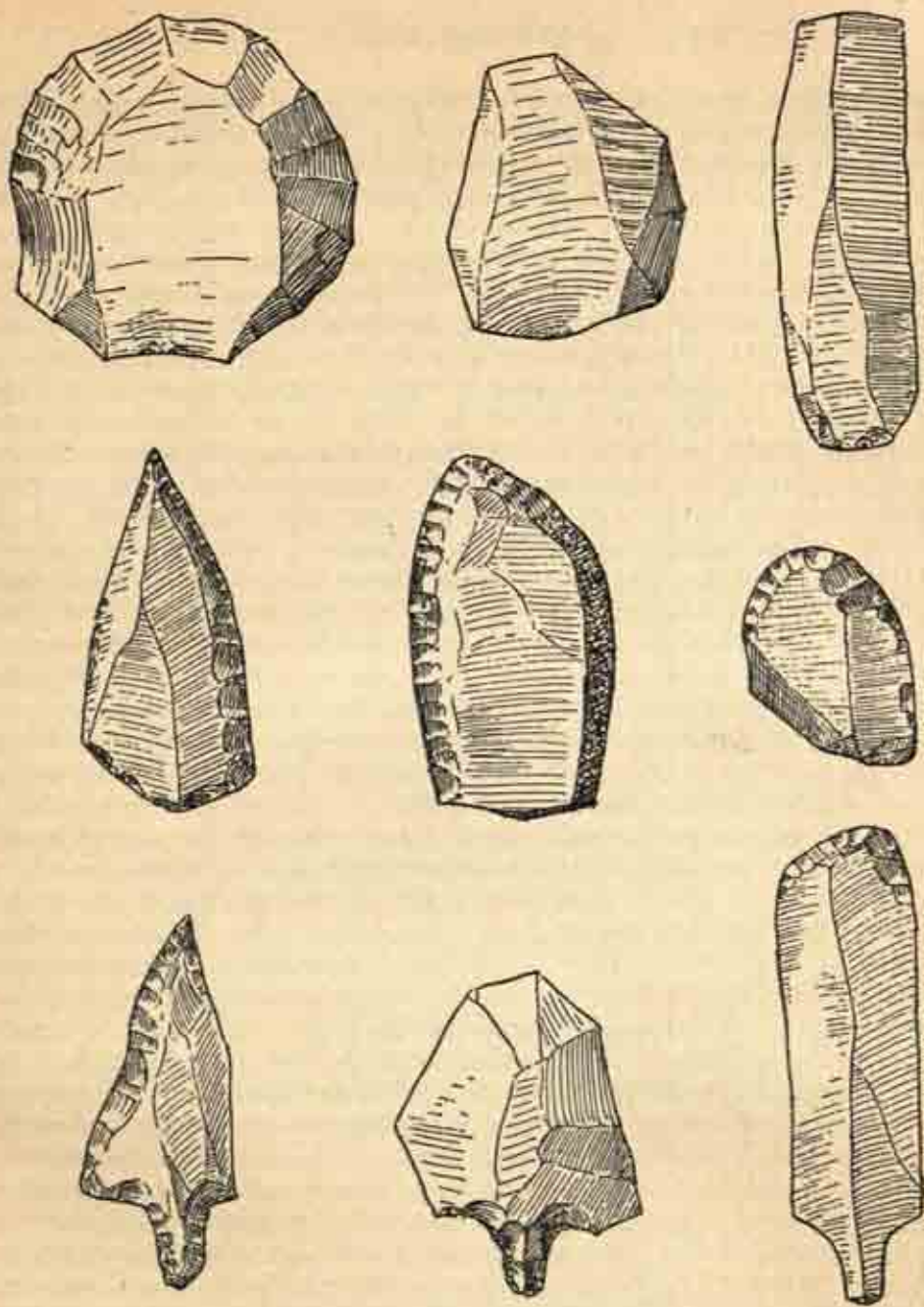


FIG. 4. BIR-EL ATER AND WAD DJOUF. Typical Aterian industry (c. 3)

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although merely a surface site, the number of specimens found enables accidental intrusions to be discounted. The similarities of chipping technique and patina equally demonstrate the uniformity of the whole culture. The site had no special attractions from the point of view of settlement; it could not have been a workshop site since there was no supply of raw materials at hand; so it was not one of those sites whose occupation is destined to last on through the ages, and where the remains of different industries are therefore found superimposed or mixed up. Thus the archaeological purity of the site is easily explained. The bulk of the industry consists of small objects which were used up completely, doubtless because of the rarity of raw material, flint and quartzite, which had to be fetched from a distance. There occur many of those discoid or round cores, finely chipped and often apparently used as implements. (These have often been explained as sling-stones). The bulk of the flakes or short blades which form the base of the industry came from these cores. Some of them have been used in their unfinished form, after a few local retouches. Others have been re-touched or re-chipped to obtain implements of the usual types: triangular points, scrapers and often tanged specimens. Without further explanations it may be said that this industry corresponds in its general principles and technique with the Aterian as exemplified in the fine deposits of the southern Constantine region. Any differences may be attributed to the raw material.

A short distance from this large Aterian site, on the edge of it one might almost say, are a series of caves inhabited at various periods.

First of all, at the bottom of a wide ravine on the coast, is the celebrated foundered cave called 'Abri Alain'. M. Pallary has conducted there a series of most careful excavations, first at his own expense, and then for Colonel Vesignié, who has a number of specimens, and for the Institute of Human Palaeontology at Paris. At the moment of writing, the archives of this foundation are publishing a monograph by M. Pallary, called 'L'Abri Alain près d'Oran, Algérie'. The industry there described and illustrated is remarkable for the small size of its implements. The cores measure 2 to 6.5 cm. in length. The majority of the worked objects consist of blades with battered backs (*à dos abattu*) in the shape of a pen-knife, or having tapering points. Also derived from the small blades are trapezes, points at the end of a blade, scrapers with side-notches. Amongst them are little round scrapers and rounded angular objects that M. Pallary regards as sling-stones. The industry belongs then, clearly, to the type which this author has called



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Ibero-maurusian (whose type-site is La Mouillah) which seems to be the equivalent, in the west of the Moghreb, of the Capsian (Getulian) in the east, and of the upper palaeolithic and mesolithic in Europe. It is in any case quite certainly very different in character from the Moustèrian-Aterian.

Now in the Abri Alain culture, occurring in a known group of wholly contrasted associations, are found certain tanged objects exactly like those of the Champ de Tir of Eckmühl. One can hardly doubt in the circumstances that this type was borrowed by the inhabitants of Abri Alain from the old industry near by, which was as 'fossil' for them as for us.

The same phenomenon seems to be met with again during the following (Neolithic) period, amongst the inhabitants of the surrounding caves. In the Grotte du Polygone M. F. Doumergue has recorded<sup>4</sup> in the black upper layer, 'plainly neolithic' and containing microliths, blades with a tongue and with notches, bone implements and pottery (p. 48 of the *separatum*), the presence of patinated flints and 'of a palaeolithic facies' which he thinks derived from the Champ de Tir site (p. 30). 'This site', he says, 'may have been for them (the occupants of the cave) a kind of reserve whence they obtained a portion of their supplies' (p. 32). And the author cites also (p. 34) 'two tanged arrow-heads of the type called Berberesque (by Pallary) or Aterian (by Reygasse) . . . obtained . . . one in the Grotte des Troglodytes . . . the other . . . in the Grotte du Polygone', as well as three others of slightly different workmanship found also in the Grotte des Troglodytes. One might object that the depths of these finds were not properly noted; but the facts remain so far as the first observations are concerned.

I myself possess a tanged object with traces of re-use, labelled 'Grottes d'Eckmühl', from the Carrière collection. It has the rather deep, slightly glossy reddish patina of the flints of the Champ de Tir; but the extremity, converted into a scraper, has been re-sharpened by some re-chipping whose fresher, more matte appearance is obvious.

Thus in these various neighbouring caves of the Polygone of Oran we see new instances of ancient stone tools re-collected by successive peoples, but with the difference that the implements thus re-used or copied have introduced a new type into the industry which took them over (FIG. 5, p. 82).

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<sup>4</sup> *Bull. Soc. de Géogr. et d'Arch. de la prov. d'Oran*, 1927 [*separatum*, with separate pagination].

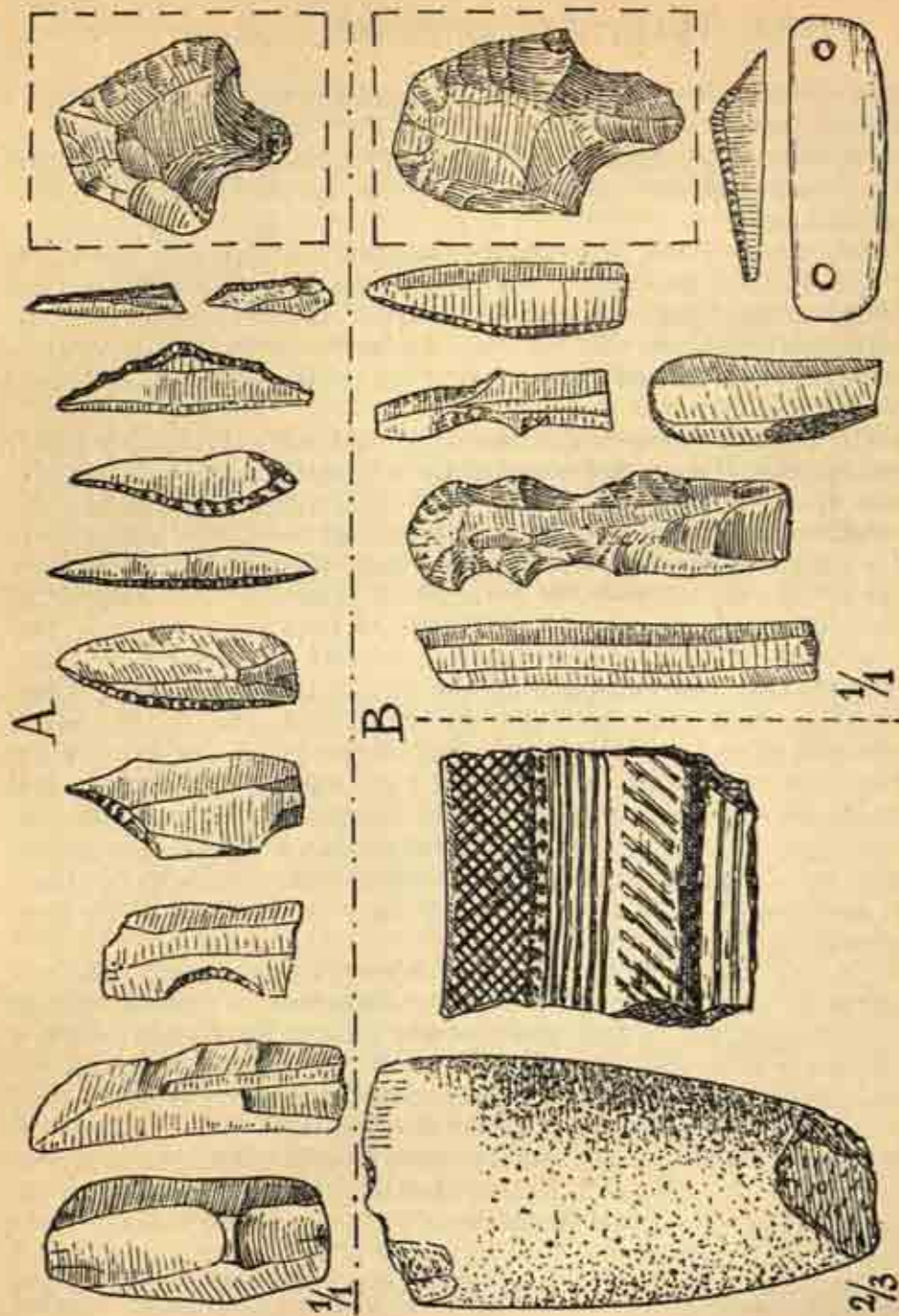


FIG. 3. A. ABRI ALAIN (ORAN). Typical Ibero-magdalenian industry, with tanged specimen inset (Pallary excavations).  
B. GROTTES D'ECHEMUHL. Neolithic industry, with tanged specimen of archaic type inset (G. Carrière excavations).



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This instance shows what errors one may fall into if one assumes, as one generally does, that identity of type indicates either direct connexion or the influence of contemporary industries. Apart from these common methods of tradition or transmission there may also occur the 're-birth' of a type through later copying by people who had no connexion with the creators of the type.

The reason why this fact has not been recognized is perhaps that metal industries contain nothing analogous. These are all of them relatively modern; they have evolved continuously in a direction parallel with needs, and usually amongst settled peoples. Then they leave much fewer traces than stone implements, and the weapon or implement became unusable through the chemical changes brought about by contact with the soil.

But if the men of our own civilization did not copy the metal implements of their forgotten predecessors, they sometimes copied the architecture of a lost tradition. One has only to see how in our French provinces Roman monuments have inspired, at the time-distance of more than a millennium, local builders who had no traditional knowledge of what they borrowed from ancient art. And everyone knows what the great period of the Renaissance owes to the influences of the monuments then unearthed.

In the realm of prehistory, most of those with any field-experience will have observed facts bearing on the present problem. By careful study of facts like these, one may perhaps succeed in explaining the remarkable resemblances sometimes observed in the same region between industries belonging to very different epochs.

## Antiquities Law, Iraq

by C. LEONARD WOOLLEY

*Director of the Joint Expedition of the British Museum and of the  
Museum of Pennsylvania to Mesopotamia*

THE Iraqi Government is proposing to supersede the Antiquities Law drawn up eleven years ago, and approved by the League of Nations, by one which will admittedly be far more onerous to the foreign excavator working in Iraq. As a prelude to the introduction of the bill before the Iraqi Parliament there has been a regular campaign of propaganda intended to show that under the existing law Iraq has been robbed, by concessions made to foreign missions, of the treasures which were legally and morally hers, and that Iraq never has had fair treatment and will not have it so long as the division of the objects from excavations is conducted by a foreign Director of Antiquities. To what lengths this campaign has been carried may be illustrated by the following: in March 1934 Abdal Rizaq Effendi, the Curator of the Baghdad Museum, personally repeated to me the statement, published in the local press, that the normal share accruing to the Baghdad Museum from the division of antiquities with a foreign mission, as conducted by the Director, was no more than one half of one per cent. of the objects catalogued. My own division had taken place a few days before; a large number of tablets and archaic seal-impressions had been reserved for division after their publication in order that the Iraqi authorities might have a better idea of the relative value of the pieces; then 75 per cent. were to go to Baghdad. There remained for division 1296 listed objects, of which the Director of Antiquities took 816 numbers, including all the best objects in each category which were selected by him before the division proper began. After the season 1932-3 the catalogue of objects comprised 516 items, of which 52 were tablets, etc., held over for division after publication, Baghdad then having the right to select 50 per cent. of the total; of the remaining 464 objects Baghdad took 302. Apart from quality, in which Iraq profited enormously by exercising the right of prior selection, the mere



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number of objects in these two years amounted to 63 and 65 per cent. of the Expedition's total collection instead of the 'one half of one per cent.' quoted by the Curator of the Museum and believed by the Iraqi Ministers and the public. The proportion has never been less than 50 per cent.

As regards the charge that foreign missions have been allowed to take away objects which ought to have been kept for Baghdad, it is of course true that there are in foreign museums, resulting from the excavations which they have financed, objects which the Baghdad Museum would rightly wish to possess, and under the terms of the existing Law it would have been possible for Baghdad to have kept them. The Law is so drafted as to allow to the Director a good deal of liberty of interpretation, and successive Directors have interpreted it in the manner which they considered best for the Baghdad Museum and for the interests of science. The interpretation could have been much more adverse to the excavators—it was indeed possible to claim for Iraq virtually everything found; had anything of the sort been done the museums would have been unable to support the excavations, work would have closed down and there would have been no Baghdad Museum. It was essential to encourage the foreign missions by conceding to them a fair proportion of objects even when the concession might be grudgingly made; such were in the nature of an advance which the gains from future work would amply repay. Again, for many years the Iraqi Government was unable or unwilling to incur any expense whatsoever in the cause of its antiquities; the Director of Antiquities was an unpaid volunteer (and not an expert) and the Museum consisted of a single room in the Serai wherein the objects were laid out on open tables; even when a special building was allotted to the Museum there was no technical staff employed. During those years the Director of Antiquities when conducting a division had to bear in mind the capacities, or the incapacities, of his Department; the Baghdad Museum could not deal with objects whose preservation or restoration involved technical knowledge and an expenditure beyond his budget: in some cases he saw himself scientifically bound to let go what he knew to be an important museum piece, but in every such case the balance was so far as possible redressed by setting against it a number of objects in good condition whose value might be not less than that of what was surrendered. The Joint Expedition was on these grounds allowed to keep the mosaic standard and the two goat statues, which the Baghdad Museum of those days could not have handled, while



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the great gold harp was restored for Baghdad at the Expedition's cost. For the same reason we had obtained the battered fragments of the Ur-Nammu stela ; but these are the only important objects whose fate was decided by considerations of the treatment which they would receive. From the time that the Government acknowledged its responsibilities and provided the Museum with a technical staff the Director has not been hampered in his selection by the question of preserving objects from destruction, and the Baghdad Museum has benefited accordingly.

There was no year in which I would not gladly have exchanged the Iraqi share of objects for that allotted to the Expedition, not even excepting the years 1927-8 and 1928-9, for at the time I did not know whether the standard and the goat statues could be restored or what merit they would have should restoration prove possible. In 1922-3 Baghdad took the diorite statue of Entemena (the best statue we have ever found), the gold figure of a priestess, half the jewellery and other objects. In 1923-4 Baghdad took the mosaic milking-scene, the finest object from al'Ubaid, as well as representative pieces of the other friezes (in each case, I think, the best piece) and the gold *bullā* inscribed with the name of A-anni-padda ; the Expedition had the foundation tablet, two copper bull statues whose restoration was problematical, and duplicates of the other Baghdad pieces. In 1924-5 the Ur-Nammu stela fragments fell to the Expedition ; Baghdad took a Kassite boundary stone and a Third Dynasty limestone relief which were otherwise the best objects. In 1925-6 our best objects were the diorite statue of Bau and a torso of Dungi, two limestone ram figures, a limestone plaque and an alabaster plaque, two fragmentary and one complete stone heads, and the stela of Bur-Sin—the last not beautiful but of historic interest. Baghdad took the Bau statue and the Dungi torso, one ram figure, the complete stone head and the Bur-Sin stela. In 1926-7 there were no very outstanding objects and the division was fairly equal. In 1927-8 (the first year of work on the Royal Cemetery) everything else was overshadowed by the gold dagger and *étui*, both of which went to Baghdad. In 1927-8 there was a vast mass of treasure to be divided ; this was done for the most part in such a way as to keep tomb-groups intact : Baghdad kept the tomb-group of Mes-kalam-dug with its gold helmet, etc., the Expedition retained that of Shub-ad ; as the latter contained many more objects a number of other groups (such as that of ' the baby princess ' ) was added to the Mes-kalam-dug group in order to give to Baghdad the superior share ; with them went the



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silver model of a boat and the embossed silver cup, the only example of silver repoussé work found; the standard was allotted to the Expedition.

In 1928-9 the Expedition profited by the Museum's lack of staff, as I have explained, in securing the two goat statues; Baghdad had the fine tomb-group with the unique gold cylinder-seal and gold tumbler, and the horned human head in bronze, also a unique piece. Since that date (when Mr Sidney Smith took in hand the organization of the Museum) the division has been much less favourable to the Expedition for the simple reason that Baghdad could now deal with its accessions; the best objects have in every case been taken and nothing has been set against them as compensation to the excavators: thus in 1929-30 the fine steatite boar of Jemdet Nasr date and the two best al'Ubaid figurines were taken; in 1930-31, out of the main finds—three limestone statues, two terra-cotta reliefs, a fragment of a third terra-cotta and a ram's head in diorite—Baghdad selected the ram's head, one relief and two statues; in 1931-2 the outstanding objects were the magnificent steatite bowl carved in relief with a procession of oxen, a granite relief of Ur-Nina and four inscribed copper cylinders; Baghdad took two cylinders, the relief and the bowl and on the subsequent division secured 323 of the 588 items catalogued, having the right of first choice in each category. It is not too much to say that as interpreted recently the Antiquities Law so far from being too lax has proved so severe that the wisdom, indeed the possibility, of carrying on excavations has been doubtful. The threat of the new law and the forecast of its terms has resulted in this, that whereas two years ago there were eleven Expeditions in Iraq this winter sees them reduced to three. The policy which produces that effect is obviously short-sighted; it has seemed well to explain how groundless is the agitation which has led to it.

An abbreviated edition of the preceding article was published as a letter in *The Times* and has been met by an article in the Baghdad newspaper *el Bilad*. The writer accuses me of ignorance of facts in that I 'asserted that the Director of this Museum receives no salary': this is untrue. I stated that 'for many years . . . the Director of Antiquities was an unpaid volunteer', which *el Bilad* admits to be true, and I referred to the change that took place when the Iraq Government acknowledged its responsibilities and provided the Museum with a technical staff.

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The newspaper quotes at length the Director of Antiquities' account of the passing of the original Law of Antiquities. With that I had not concerned myself: my point was that the existing law as dealing with foreign expeditions fully safeguarded the interests of Iraq, so that in those respects no change was desirable.

The Director of Antiquities is represented as saying that, with regard to the distribution of objects (between the Iraq Museum and foreign missions) he had made a study of the facts but had been unable to obtain precise figures offhand. I cannot understand his inability, as lists are filed in the Museum of each season's finds made by each Expedition, and on those lists the allotment of each individual object is noted. The figures quoted in my own article are taken from the duplicate lists in the possession of the Ur Expedition.

The Director of Antiquities added 'Don't be surprised when I tell you that the number of objects lent to the mission excavating at Ur alone exceeds five thousand'. These are tablets and inscribed objects not yet definitely allotted, the publication of which has been entrusted to the Expedition, and, as I stated, and the Director agrees with me, distribution of them will be made after publication: then Iraq will select, according to signed agreements, in some cases 50 per cent. and in some cases 75 per cent. of the totals.

Not a single statement made in my letter has been met or refuted by the *el Bilad* rejoinder. I should only add that my judgment of the new Antiquities Law was based on a summary sent out by the Antiquities Department itself to excavators, and the existence of such a draft is ignored and even implicitly denied in the Arabic article. Lastly I said that the expeditions working in Iraq would be reduced this year 'to three or even to two'; *el Bilad* says that work is going on in four regions—this is correct, but two of the sites mentioned are worked by one Expedition—that of the Oriental Institute of Chicago—and only three Expeditions are in the field.



## Notes and News

### SUPERIMPOSED CULTIVATION-SYSTEMS (PLATE I)

When I first described the Celtic field-systems so admirably revealed by air-photography, I mentioned one instance where it seemed that that system was overlaid uncomformably by another.\* There was not then time to examine the site—which is remote and difficult of access—nor was the only air-photograph then available good enough for detailed study. Since then others have come to hand, and I have walked over the ground, marking in on the photograph (PLATE I) certain features which it does not show. The result confirms my hypothesis ; but the absolute age of each system remains unproven.

The area is on Thornham Down, and includes the southwestern-most portions of the parishes of Rushall, Upavon and Enford (Wilts, 47 NW). Archaeologists will recognize its position when I say that it is 1½ miles southwest of Casterley. As it falls within the danger-zone of the artillery ranges, it can only be visited on certain days ; and as there are no made roads within two miles, one has to walk there. In spite of the intermittent bombardment the region has undergone for so many years, the permanent damage done is surprisingly small ; indeed more harm has been done by the burrowing of rabbits which here, as everywhere on War Department land, have bred and multiplied inordinately.

The older system consists of a series of ' ladders ' standing, so to speak, upon Old Ditch. The component parts were small rectangular fields of a common type, with some very pronounced lynchets. The biggest of these still stand out very clearly, throwing a strong shadow. They have survived even where the long swathe-like ridges of the later system have obliterated the others, leaving but a faint undulation visible. In places one can perceive the ribs of this older system showing up through the other like the bony skeleton of an old horse. At certain points (marked A in the explanatory diagram), the intersection of the two is very plain, both on the photograph and on the ground ; on the

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\* *Air Survey and Archaeology*, 1st edn., 1924, plate v, p. 22.

## ANTIQUITY

ground the later age of the 'swathes' is very plainly seen, for they cut right into the older lynchets.

There can be little doubt that the later 'swathe' system is of Saxon or Medieval date. The strip-like character of the cultivation alone would suggest this; but even stronger evidence is to be found in the fact that genuine strip-lynchets occur on the slopes of Water Dean Bottom, extending almost continuously as far as the mouth of the Dean at Compton. It is difficult to dissociate these from the cultivation-system here shown, which seems to be an extension of the Compton group. If they are Saxon or Medieval then this photograph is an example of what has long been desired, namely, of the strip-system superimposed upon an earlier Celtic one. Evidence of early Saxon occupation has been found at West Chisenbury in the Avon Valley, only  $2\frac{1}{4}$  miles to the northeast of Thornham Down. Here, finds indicating a pagan Saxon cemetery were found in July 1928 (*Wilts. Arch. Mag.* XLV, 84). Such finds are notoriously rare in Wiltshire. Waterdean Bottom (called Waterdeane in 1591) is one of the deepest combes of the Plain, with perennial flowing water in its lower portion. It would therefore have attracted settlers in all periods, particularly the Saxons.

The cultivation-terraces at Compton have already been illustrated (*Air Survey and Archaeology*, 2nd edn., 1928, plate VI B).

It will be observed that although the later cultivators have occasionally run their ploughs diagonally across the older rectangular Celtic field-system, elsewhere they have respected its lines. That was to be expected; and no doubt it happened in every region where the one system impinged upon the other. It certainly happened, for instance, along the slopes of the lower Avon Valley (see Ordnance Survey 'Map of Celtic Earthworks of Salisbury Plain', scale 1:25,000, Old Sarum sheet, published 1934); and probably throughout the chalk-region of Kent. Commonsense would ordain that the bigger, steeper lynchets should be adopted as the boundaries of the later fields; whereas the smaller ones could be ploughed over and ultimately levelled.

It will be noticed that the Celtic system is aligned upon Old Ditch, a linear earthwork consisting, where best preserved, of a low bank between two ditches.

Another instance of strip-ploughing upon an older rectangular Celtic system occurs on Great Litchfield Down, near Ladle Hill, North Hants. (Ordnance Survey air-photograph 'Ladle Hill 2144').

O.G.S.C.





PLATE I



AIR-PHOTOGRAPH OF THORNHAM DOWN, WILTS. SHOWING TWO SYSTEMS OF CULTIVATION SUPERIMPOSED. (See p. 89)



# NOTES AND NEWS

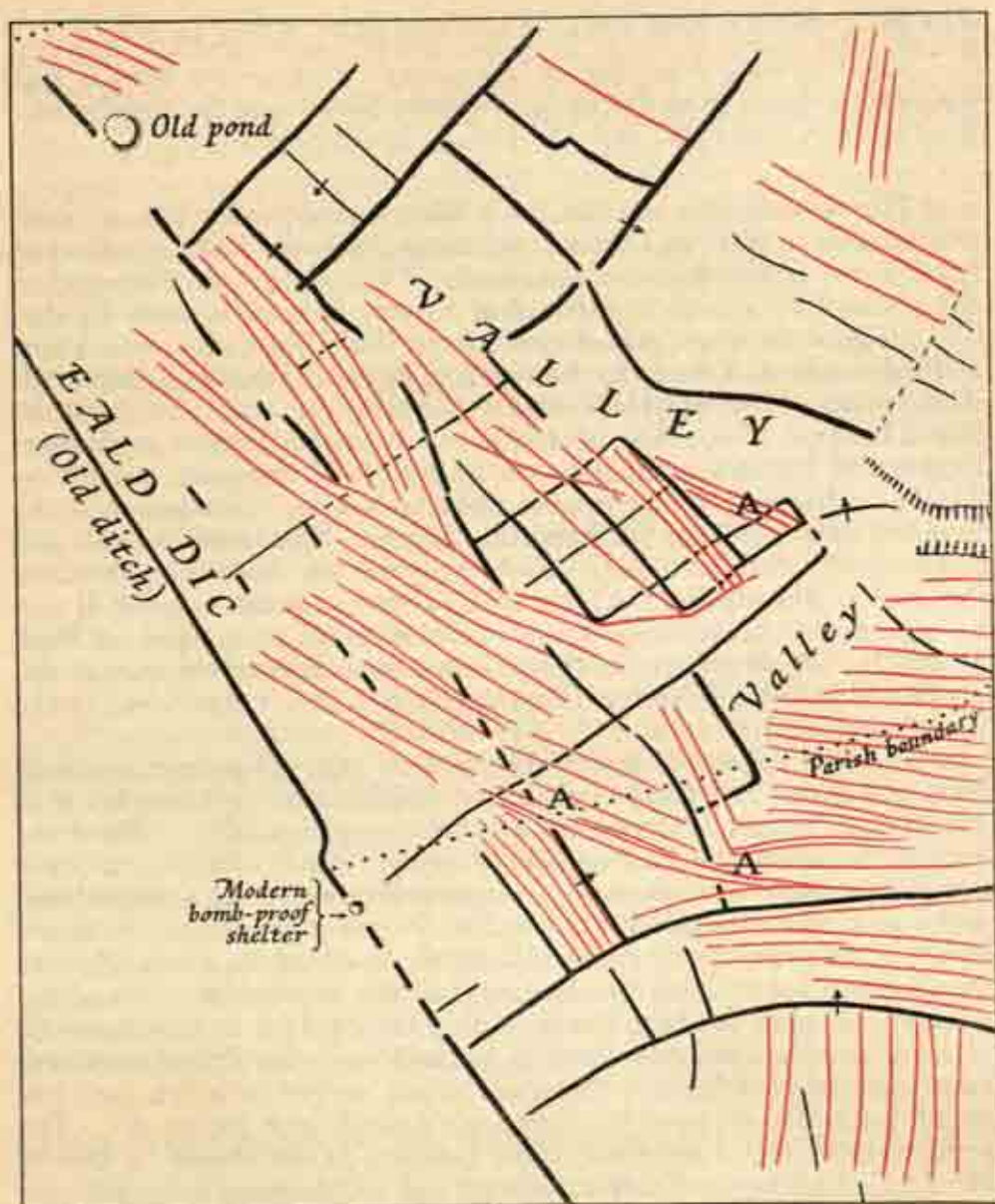


DIAGRAM TO EXPLAIN AIR-PHOTOGRAPH

The black lines are the boundaries of the outer (Celtic) field systems; the red lines the ridges of the later (Saxon or medieval) strip-system. Arrows indicate direction of slope.

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### RECENT WORK ON HADRIAN'S WALL (PLATE II)

The following summary of last season's work on the Roman Wall has been received from Mr F. G. SIMPSON, Director of the Cumberland Excavation Committee, and Mr IAN A. RICHMOND :—

The investigation of Hadrian's Wall, whose eighty Roman miles bristle with major and minor problems, receives little attention in ANTIQUITY, largely because the results of the steady work devoted to this greatest of Roman frontiers year by year is to be measured rather by quinquennial than annual reckoning. Five years ago, excavations at Birdoswald (summary in ANTIQUITY, 1930, IV, 102-4), marked the culmination of the efforts directed to defining the main periods of the Wall's history. Since then, efforts have been directed for the Cumberland Excavation Committee in the west by Mr F. G. Simpson, assisted by Mr I. A. Richmond and others, towards solving the relation of the Stone Wall to the Turf Wall and the Vallum. The latter question still awaits final solution, though steady progress has been made towards that goal. The place of the Turf Wall in relation to the Stone Wall may be regarded as settled conclusively by the recent excavations at High House Turf-Wall milecastle, the fiftieth milecastle from the east, which is situated in the only sector where the Turf Wall is not overlaid by the later Stone Wall.

The new milecastle resembled those of stone in pattern, but had ramparts of turf and gates of timber. A single internal barrack was of wood, and yielded evidence for a short occupation only. The front gate of the milecastle had supported a tower, overlooking a causeway across the ditch—a causeway of virgin soil, pierced by a wood-lined gulley to prevent a wash-out in the ditch on the steep slope. This and all other woodwork was dismantled at the close of the occupation, as shown by chips of sawn timber found in the two rubbish pits of the demolition-squad. These pits were dug through the occupation-earth of the milecastle, and one of them contained a fragment of the dedicatory-inscription of Hadrian and Platorius Nepos, carved on a two-inch oak board, once placed above the milecastle's north gate (PLATE II). The restoration of this inscription, from portions of six letters, is due to Mr R. G. Collingwood, and represents one of the most exciting contributions of the skilled epigraphist to mural research. The result is to date the Turf Wall to the governorship of Platorius Nepos in A.D. 122-126, and to class it as part of the same scheme as the Stone Wall,



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already being built further east. The combined scheme, partly in turf and partly in stone, is the result of Hadrian's own visit to the province.

Meanwhile, the length of the Turf Wall has been determined by following up the discovery, in 1927-8, that this wall had a special type of stone turret, easily distinguishable from turrets belonging to the Stone Wall by its special architectural features. These turrets, later embodied in the Stone Wall, have been traced from Birdoswald far into Cumberland, and the end of the trail was reached in 1934, when turret 79A, the last on the wall, proved to be of Turf-Wall type. While the west limit of the Turf Wall is thus known, the east limit is marked by the cessation of the Turf-Wall type of turret at the Irthing, east of which the turrets at once conform to Stone-Wall type. Hadrian's Wall emerges as a composite work, of which 49 miles were built in stone, and 31 in turf: and the reason for the employment of turf in the west is to be connected with the entire absence in those regions of local supplies of lime, used in great quantity for grouting the masonry structure.

While the discoveries at High House milecastle show that the Turf Wall was rapidly replaced by the Stone Wall in this sector, it remains uncertain whether this was true throughout. Turret 54A produced, in 1933, evidence for a reconstruction of the Turf Wall after an already considerable occupation, while showing at the same time that the Stone Wall was built before the disaster of A.D. 196. Again, evidence exists to show that the 'Turf' Wall was in this very sector built largely of beaten clay. These facts, while not affecting the original date of the Wall, suggest that further digging will produce and solve interesting minor problems. Detailed annual reports of this work have been regularly published in the *Transactions of the Cumberland and Westmorland Archaeological and Antiquarian Society*, which has long been responsible for western investigations on the wall; and an extended summary of the evidence relating to the Turf Wall is to appear in a forthcoming volume of the *Journal of Roman Studies*.

### ROCK-MARKINGS IN SOUTH AUSTRALIA (PLATES III, IV)

Mr A. L. Meston's illustrated article (*ANTIQUITY*, 1934, VIII, 179-84) on rock-carvings in Tasmania is of much interest to workers in South Australia, where since 1902 many markings of similar type have been recorded. Some account of these will be found in the papers, in



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which additional references are given, mentioned in the footnote.<sup>1</sup> In these papers rock-carvings of several types and of varying ages are described; their detailed classification has not yet been attempted. Those of special interest are chiefly found carved on outcrops of slate and quartzite rocks in the arid and semi-arid regions of the Flinders Range, Yunta and Mootwingee districts; although we also noted similar carved circles, etc. during the Adelaide University Anthropological Expedition to Macdonald Downs at Undala, Central Australia.<sup>2</sup>

Mountford's summary (*l. c.*, 1928) of them is an excellent one. The designs include circles, lines, animal tracks and outlines of animals.

There has always been a tendency amongst South Australian recorders to regard this type of carving as surviving from 'antiquity' or 'great antiquity'. The evidence deduced has been for the most part quite general. It may be accepted tentatively as an 'opinion'. None of the surviving Australian tribes has claimed ownership of them and no suggestions of carvings made in modern times have been noted except a statement by a civilized native at Owicandana, who vaguely attributed some marks to his 'grandfather'. (Hale and Tindale, *l. c.*, 1925, p. 55).

The accompanying photograph (PLATE III) shows a typical series of South Australian rock-markings of a type similar to those recorded from Tasmania by Mr Meston. The resemblance is particularly close to those records from Mount Cameron West, a locality ascribed in error on the plates to New Zealand.<sup>3</sup> This photograph was taken at Middle Waters, Wilpena, South Australia; the outlines having been filled in with white clay to make them more apparent. PLATE IV shows a more elaborate drawing from Warrawarumby Creek, three miles north of Panaramittee Station in the Yunta district of South Australia. Associated with circles and other figures there is the outline of the head of a crocodile, a type of animal now extinct in South Australia, although its remains are found in Pleistocene and perhaps in early Recent beds.<sup>4</sup>

<sup>1</sup> Basedow, *Journ. Roy. Anthropol. Inst.*, 1914, XLIV, 195-211, pls. 1-XVII; Hale and Tindale, *Records S. Aust. Museum*, 1925, III, 52-56; and 1930, IV, 208-11; Campbell, *Trans. Roy. Soc. S. Aust.*, 1925, XLIX, 123-7, pls. XI-XII; Pulleine, *Trans. Roy. Soc. S. Aust.*, 1926, I, 180-2, pls. XXV-XXVIII; Mountford, *Rept. Aust. Assoc. Adv. Sci.*, Hobart, 1928, XIX, 337-66 (bibliog.); Hale and Tindale, *S. Aust. Naturalist*, 1929, X, 30-34.

<sup>2</sup> Tindale, *Trans. Roy. Soc. S. Australia*, 1931, LV, 37.

<sup>3</sup> This unfortunate slip has been duly noted in our corrigenda to volume VIII.—EDITOR.

<sup>4</sup> There is a plaster cast of this carving in the South Australian Museum, and copies of it and many others are available for exchange.



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The carvings at Mount Cameron West appear to possess so many affinities with the markings of similar type from South Australia that they must be regarded as another link in the growing chain of material evidence suggesting that a Tasmanoid people once lived on the Australian continent, as well as on its Tasmanian peninsula. Implements of Tasmanian facies have been found in more than one place in New South Wales and South Australia. Upon the arrival of the newcomers, the Australoids, these autocthonous died out save in the refuge area provided by the cutting off of Tasmania as an island, a result of post-glacial changes in sea level. The actual process of replacement of Tasmanoid by Australoid may well have been similar to that large-scale operation today whereby the Australian aborigines themselves are vanishing before contact with Europeans, leaving minor traces in the language of the newcomers and a few people of mixed blood who tend to merge into the mass of the European population.

NORMAN B. TINDALE.

### RAG-WELLS (PLATE V)

In the introduction to his book *Legendary Lore of the Holy Wells of England*, published in 1893, Mr R. C. Hope writes :—' The hanging of rags and scraps of clothing on the branches of trees, and on bushes about the Holy Wells, is probably a remnant of the old tree-worship ; it obtains all over the globe ; it is very common in Great Britain '. The photograph (PLATE V) taken during the summer of 1934 shows a ' Rag well ' known as St Helen's well in the parish of Walton, West Riding of Yorkshire (Yorks, 189 NE). This well is given twice in Hope's book under Newton Kyme and Thorpe Arch, two neighbouring parishes ; but despite the statement quoted that rag-wells are very common in Great Britain only four others, Gargrave (Yorks), Newcastle and Benton (Northumberland), and Great Cotes (Lincoln) are given in his catalogue of 450 holy wells. The custom of hanging rags on the bushes was already extinct at the first three of these when Hope wrote. It therefore seems likely that a functioning rag-well may be sufficiently rare to merit illustration and description in *ANTIQUITY*.

St. Helen's well is situated in a little copse called Chapel Wood on the east side of the Roman road known as the Rudgate, which runs north and south and crosses the river Wharfe at St. Helen's ford. The well is 190 yards north of the north bank of the river. There is now no well or visible spring, but from the position at the lower margin of a gravel terrace it is obvious that water would be obtainable by digging



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a few feet ; a small stream flows just east of the site. The Roman road is well authenticated ; in later times a chapel, dedicated according to Leland to St. Helen, stood in Chapel Wood between the track of the road, here a grassy lane, and the well. Fragments of an early sculptured cross were found here ; an illustration of these fragments from Whitaker is reproduced by Bogg in his *Lower Wharfedale*, where there is also a photograph and description of the well in 1900.

It is curious that the hanging of rags should survive when the actual well has vanished, but the writer has visited the spot many times in the last seven years and there are always plenty of obviously recent additions. The custom is to stand facing the well (*i.e.* due west) preferably after sunset, wish, and then attach something torn from one's clothing either to the big tree—a wychelm—or to any of the bushes. Probably the custom is largely maintained by vagrants who frequently camp in the wood, but it also has its attraction for courting couples from the neighbouring villages !

The writer has been informed by Mr C. Shillito, agent of the Brocklesby Estate, that the 'Healing Well' near Great Coates (spelt Cotes by Hope) is now in a neglected state, but that 30 to 40 rags are hanging on the bushes. He has also enquired of his colleagues on the Geological Survey, but not many instances of rag-wells are known to them. Mr Dewey mentions one in Beddington Park, Surrey, as still functioning, and quoted St. Mary's well, Boxwell from *Ancient Wells, Springs, and Holy Wells of Gloucestershire* (1928), by R. C. S. Walters. But in September 1934 this well was utterly deserted though an aged labourer on the estate 'had heard tell' that visitors used once to hang rags there.

The photograph is by Mr J. V. Stephens of the Geological Survey,  
York. C. N. BROMEHEAD

## THE HOGA OF CUTTESLOWE

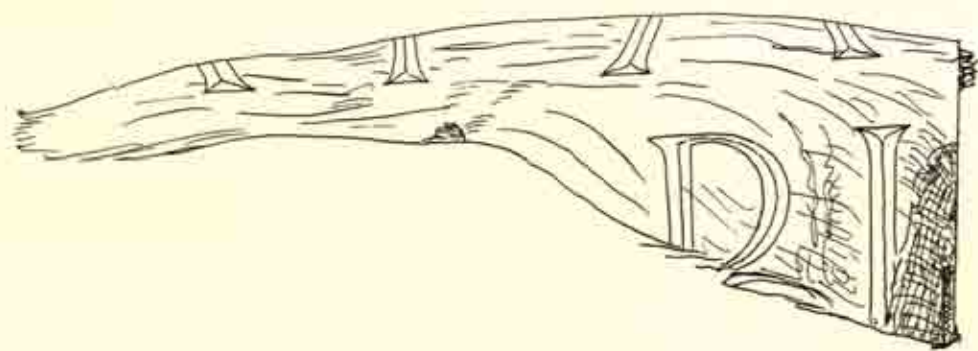
On the roll of the justices in eyre who sat at Oxford in the year 1261 (Public Record Office, Assize Roll 701, m. 22) is an entry of some archaeological interest :—

Duo homines extranei inventi fuerunt occisi sub hoga de Cudeslowe. . . Et testatum est per xii quod malefactores latitant in concavitate illius hoge, et ibi plures roberie et homicidia fuerunt. Ideo preceptum est vicecomiti quod prosterni faciat hogam illam.

TRANSLATION. Two strangers were found killed under the 'how' of Cutteslowe. The hundred jury testify that evil doers



PLATE II

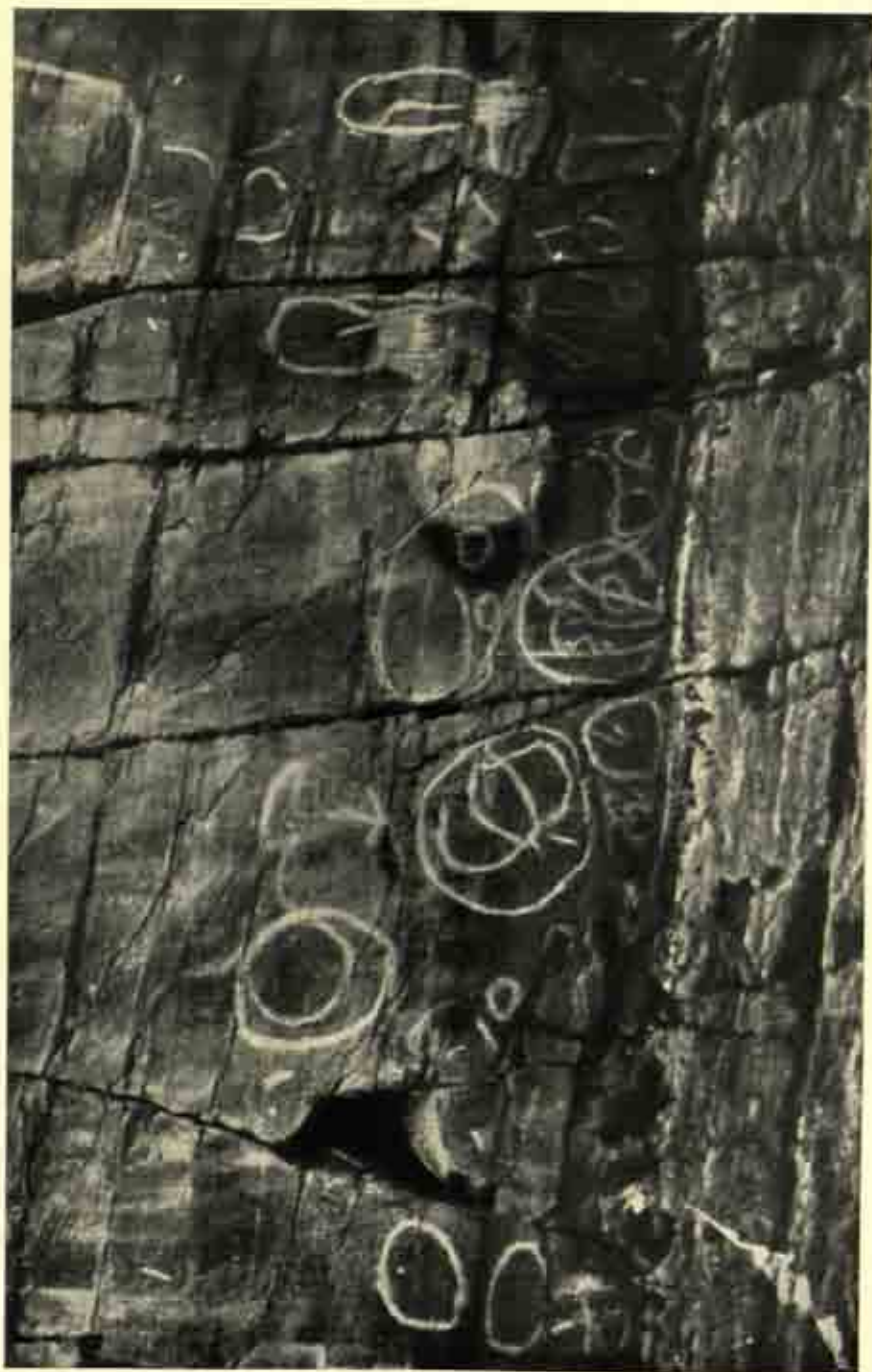


IMP·CAES·TRAIANI  
HADRIANI·AVGVSTI  
LEG·II·AVG·P·LATORIO·NEPOTE  
LEG·AVG·PR·PR

INSCRIBED TIMBER FROM TURF-WALL MILECASTLE AT HIGH HOUSE, WITH RESTORATION BY  
MR R. G. COLLINGWOOD. (See p. 92)

*facing p. 96*

PLATE III



ROCK-MARKINGS AT MIDDLE WATERS, WILPENA, SOUTH AUSTRALIA. (See p. 94)  
*Ph. S. F. Ashland*



PLATE IV



ROCKMARKINGS AT WARRAWARUMBY CREEK, NEAR PANARAMITTEE STATION, YUNTA DISTRICT, SOUTH AUSTRALIA. (See p. 94)  
Ph. S. F. Ackland



PLATE V

RAG WELL, KNOWN AS ST. HELEN'S AT WALTON, NEAR WAKEFIELD, YORKS. (See p. 93)

Ph. J. V. Stephens



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are wont to lurk in the hollow of the 'how', and that many robberies and homicides have been committed there. Therefore the sheriff was commanded to level the 'how'.

Cutteslowe, today a part of Wolvercote, but formerly a parish of itself, in Wotton Hundred, lies immediately to the north of Oxford, between the Cherwell and the main road running from Oxford to Banbury. In Domesday Book five hides are attributed to Codeslau, two held by the canons of St. Frideswide, and three by Roger of Ivry [*Dd.* 1, 157, 159]. A St. Frideswide's charter, attributed to 1004, includes the *portstret*, that is, the Banbury road of today, in the boundaries of its holding at Cudeslowe [*Cartulary of St. Frideswide*, ed. S. R. Wigram, 1, 4]; and the Rev. H. E. Salter kindly pointed out to me that if the *hoga* was near the high road, it would be very handy as a hiding-place from which to attack wayfarers. Of the two Cutteslowe farms today, the northernmost, St. Frideswide's, the property of Christchurch, immediately adjoins the Banbury road, and presumably represents the canons' Domesday holding. There is a very fine old farmhouse, with a moat, but the farmer, Mr A. B. Kerwood, informed me that so far as he knew, no ancient remains had been found on his land and, as is perhaps only to be expected after 670 years, there are no obvious signs of any mound or rising in what is, on the whole, a very flat stretch of land. There seems no doubt, however, that the mound which the sheriff was ordered to level in 1261 was the barrow or *hlæw* which originally gave its name to Cutteslowe. HELEN M. CAM.

Miss Cam's discovery raises a whole number of other points, some of more than merely local interest. It may be said at once that the exact site of the 'hoga' cannot now be identified, but that it may well have been very near to, if not actually on, the site of the present farm, the northernmost of the two. (This, as Miss Cam has said, is the older; it was called 'Old Cutchlow' on the Ordnance Map of 1814). Now there are actually two *round* barrows about 400 yards west by north of 'Old Cutchlow'; though much ploughed over they are still plainly visible both on the ground and on an air-photograph. There is another a little further west, near spot-height 250 (Oxon. 33 NW, edn. of 1922).<sup>1</sup> The two round barrows first mentioned may well be the

<sup>1</sup> I have no doubt that there were many more along this gravel ridge between Thames and Cherwell. Major Allen found and photographed two barrow-circles on the Keble College cricket-ground last summer; and there is a round mound which may be a barrow on Port Meadow.

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'twam lytlan beorgan' of the bounds of Eaton (BCS. II, no. 607, A.D. 900 for 904), if, as I believe, that place is to be identified with Water Eaton rather than with Wood Eaton. (I cannot discuss this now, but would point out that the mention of Wifeles Lace both in these bounds and in those of Cutslow of A.D. 1004 proves that the two townships adjoined one another). In any case there is thus both documentary and field evidence of barrows at Cutslow, though so far only of round ones; and for a robbers' den we require a stone-chambered long one! We need not go far to find an example, nearly ploughed out but authentic—the 'langan hlæw' of A.D. 1005 described in *ANTIQUITY* 1930, IV, 357. This long barrow lies exactly 5 miles northwest of Cutslow, by the side of the same ridgeway (the 'port-stret'). From the stoney nature of what survives it is reasonable to suppose that it was originally a stone-chambered structure like the Cotswold ones. Further, Mr A. H. A. Hogg has found what he believes to be yet another long barrow  $\frac{1}{2}$  of a mile to the southeast of it.

It seems therefore that there may have been as many as three stone-chambered barrows on the lowland ridge running parallel to the Cherwell on the west. That would be in complete accordance with what we now know about the topographical fancies of prehistoric man—his love of gravel and limestone and his dislike of heavy clay soils.

For the moment, however, what is required is some old estate-maps of the parishes of Water Eaton, Wood Eaton, Cutteslowe (as it is spelt on the O.S. map) and Gosford, so that the bound-marks already quoted may be identified with certainty.<sup>2</sup> O.G.S.C.

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<sup>2</sup> I should like to thank Mrs Cunningham who first called my attention to the subject here discussed.—O.G.S.C.



## Recent Events

*The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.*

A battle in A.D. 821 is, perhaps, hardly a 'recent event', but it seems at any rate to be 'news', since the last record of it (so far as known) was in Leland's *Itinerary* (ed. L. Toulmin Smith, 1908, II, 151-2). The passage runs as follows:—

'A<sup>o</sup>. D<sup>i</sup>. 821. fuit grave bellum inter Egbertum regem West-Sax. et Ceolwlfum regem Merciorum inter Abingdon et Oxford in loco qui Cherrenhul dicitur, victore Egberto'.

~ ~ ~

No other record of this battle has survived. Leland derived his information 'ex libro Universitatis Oxon.', so that there is good hope that the source from which he took it may still survive, either in the Bodleian or in some college library or muniment room. The name, which should appear now in some such form as Charnhill, is not known to those most familiar with the history of the region. On the other hand, as Dr Salter has pointed out (in a letter), Leland does not make it clear whether he himself knew the place to be between Abingdon and Oxford, or merely that his authority said so.

~ ~ ~

Another interesting fact recorded by Leland (edn. 1907, I, 50), is that Hull was paved with stones imported as ballast from Iceland:—  
'At such tyme as al the trade of stokfisch for England cam from Isleland to Kingeston, bycause the burden of stokfisch was light, the shipes were balised with great coble stone brought out of Isleland, the which yn continuance pavid al the toun of Kingeston throughout'.

~ ~ ~

Iceland is mainly formed of volcanic rock, and the cobbles, if any still survive, should be easy to identify. The point is of interest to

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geologists, and shows that 'foreign' stones, even of large size, may occasionally be transported by human agency. Geologists, of course, have always been fully aware of this; we cite this instance merely because it is not often that such circumstantial details are set on record.



What seems to be the remains of an early system of field-walls has been revealed by air-photographs at Cush, near Kilfinane, co. Limerick, where Mr Seán O'Riordain has been excavating. (*Irish Press*, 19 November 1934).



In a lecture at Belfast Dr Mahr, Director of the National Museum of Ireland, suggested that the custom of living in pile-dwellings above water (though sometimes also on dry land) originated in Central Europe and spread outwards to the marginal regions; and that in these it lingered on long after it was abandoned in the lands of its origin. The theory is an attractive one and seems in agreement with most of the known facts; but it appears from the discoveries at Ehenside Tarn in Cumberland that pile-dwellings were already known in Britain in the Neolithic period. (Report in the *Belfast News-Letter*, 6 December 1934; see also *Arch.* XLIV, 273 and *Arch. Journ.* 1931, LXXXVIII, 143).



The earthworks at Stanwick in Northamptonshire have again come under discussion. In an unguarded moment the writer of these notes once suggested, without having seen them, that they might be no more than the remains of a medieval deer-park. He takes this opportunity of withdrawing that opinion. After flying over them for some time and consulting air-photographs taken subsequently, he has no doubt that they are of prehistoric origin, comparable to those near Verulamium. This interpretation is supported by the well-known bronze objects from there, now in the British Museum. (*Northern Despatch*, 24 October 1934).



An Expedition is at work excavating 'a settlement of the early Byzantine Age in Nubia'. (*Morning Post*, 18 October 1934). We hope that some day that site of first-class importance, the town of



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Soba on the Blue Nile, some 12 miles above Khartum, will be dealt with by a fully qualified archaeological expedition. It is a plum, and is, we understand, practically untouched. The only other excavations, conducted in that region more than 20 years ago, are still unpublished; but they revealed the existence of an otherwise unknown contemporary culture of some pretensions.



Excavations at the hill of Mapungubwe, conducted by Pretoria University, have resulted in the discovery of many gold objects. (*African World*, 13 October 1934).



Every lecturer knows, and fears, the local press-reporter who asks for a summary of the lecture (so that he may not be obliged to remain and compile one himself). Those who refuse do so at their own risk. Amongst them was, presumably, the lecturer on the Evolution of Roman Architecture who was reported (13 October 1934) as saying that 'the Etruscans had developed a splendid civilization in Italy by 8000 B.C.'. One might attribute the extra cipher to a printer's error, had not the column been headed 'Modernism 10,000 years ago'!



A Swedish Expedition led by Dr Olaf Janse is to excavate ancient cemeteries of the Hun Dynasty in Yunnan. (*Birmingham Daily Mail*, 29 October 1934).



A new room has recently been opened in the Baghdad Museum, devoted mainly to the objects excavated at Warka (Erech) by the German Expedition. (*Morning Post*, 12 December 1934).



St. Sophia has now been closed as a mosque, after having been used as such ever since the Ottoman conquest in 1453, and is now to become a museum of Byzantine Art. (*The Times*, 11 December 1934).

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The outstanding success of the Fenland Research Committee since its inception in 1932 has indicated the need for similar coordinating organizations elsewhere. The latest recruit is Norfolk, where a Research Committee was inaugurated at the end of September. It consists of slightly over fifty individuals of whom one-third are resident outside the county. The Norfolk Research Committee has adopted the same wide terms of reference as its prototype. The field of investigation comprises the whole county except the Fens in the west, which belong to a different natural region. The county boundaries will not be adhered to rigidly. On the archaeological side the committee hopes to excavate some of the sites already revealed by air-photography. The work of Sir Cyril Fox on the distribution of man in East Anglia has shown the significance of the area, and has outlined its problems. The committee will devote itself to a solution of these and kindred problems in geology, geography, plant ecology, social and economic history, palaeontology and climatology. No separate publications will be issued; the results will be published by individuals or groups of individuals in the appropriate journal. The cooperation of all interested in the problems of this area of East Anglia is invited. Communication should be made to the Hon. Secretary, Mr Rainbird Clarke, c/o The Castle Museum, Norwich. Financial assistance is needed urgently and contributions to the funds will be gratefully received by the Hon. Treasurer, Mr C. R. A. Hammond, Barclays Bank, Norwich.



At Doring in the 'region of the Tibetan lakes' there have been found alignments of standing stones 'comparable in every respect with those of the Morbihan, except in the larger number of the parallel rows which are eighteen. As in the Morbihan, they appear to end (in the west) at a stone circle, enclosing several standing stones in front of which is a stone table'. (*L'Anthropologie*, 1934, XLIV, 627 reviewing 'Sur les pistes de l'Asie centrale', par G. de Roerich: Geuthner, 1933).



A geographical study of '“hommes supérieurs” du Portugal (les militaires et les hommes politiques étant systématiquement exclus)' reveals the fact that they are closely associated with intellectual centres. Racial and environmental factors seem to have little or no connexion.



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(*L'Anthr.* 1934, XLIV, 634, reviewing A. A. Mendes Correa, 'La distribution géographique des Hommes supérieurs'. *Rev. Anthr.* 1933, XLIII, separate pagination).



Monsieur Favret has been given a grant to excavate the grottes of the Marne (*L'Anthr.* 1934, XLIV, 683).



An article by Professor Maiuri on the past year's excavations at Herculaneum and Pompeii is printed in *The Times*, 30 October 1934, p. 13. In this he deals with the reconstruction of architectural remains found in previous years, which as he says must be replaced as far as possible in their original settings.



A fine example of a Roman cauldron, or camp kettle, has been recovered from the bed of the river Ouse, at York. It has special interest because of two inscriptions, or marks of possession, on the inside of the rim. The lettering is punched in a series of dots and is deciphered by Mr R. G. Collingwood as 'Centuriate of Attius Severus and Centuriate of C (?) Aprilus. The date of the cauldron, which is in very fine preservation, is considered as 1st century (*The Times*, 20 December 1934, p. 17).



The warlike tendencies of Primitive Man have been discussed in letters to *The Times* by Mr Gregory Bateson (13 December, p. 12) and Professor Elliot-Smith (20 December, p. 8) in reply to one by Dr William Brown (11 December, p. 12) on 'Psychology and War'.



The principles of Hellenistic architecture are considered in an article in *The Times* (8 December, p. 13) by Theodore Fyfe, Director of the School of Architecture in the University of Cambridge, based upon recent archaeological discoveries, particularly in Transjordan.

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An account of a farm settlement of Roman times, and of the courtyard houses attached, found at Caerau Farm, near Pant Glas station in Wales, is given in *The Times* (29 December, p. 7) by Mr B. H. St. J. O'Neil, of H.M. Office of Works, who sums up his report as follows:

'It seems clear that this ancient farm and, doubtless, others of the type in the district, are the fruits of the *Pax Romana*, and of the encouragement given to agriculture, which must have been one of its principal features'.

The recent article in ANTIQUITY (December 1934) by Sir Cyril and Lady Fox on the forts and farms on Margam Mountain, Glamorgan, gives additional interest to this farm-settlement.



The Rome correspondent of *The Times* has received a report (printed 1 January 1935, p. 13) from Dr Luigi Ugolini,\* Director of the Italian Archaeological Mission in Albania, of the most recent results of the excavation of Butrinto (Buthrotum) on the Albanian mainland opposite Corfu. Various constructions connected with the sanctuary of Aesculapius have been found, and also a fountain, with frescoed niche which may have been a shrine dedicated to the god of medicine. There are also some finely sculptured votive pillars and inscriptions, including one recording the honorary freedom of Buthrotum conferred upon a citizen of Corfu.

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\* Dr Ugolini's just published book on Malta will be reviewed in the next number of ANTIQUITY.



## Some Recent Articles

*This list is not exhaustive but may be found convenient as a record of papers on subjects which are within the scope of ANTIQUITY. Books are occasionally included.*

Some notes on the Biggo bya Mugenyi [printed Bagenyi], by E. J. Wayland. *Uganda Journal*, vol. II, 21-32.

Notes with plans of defended sites and what appear to be earthworks designed to conserve water.

An Attempt to Cross-date Trees in Drowned Forests, by Charles J. Lyon and James W. Goldthwait. *Geographical Review*, October 1934, pp. 605-14.

The authors have investigated selected trees from the submerged forests of New England and Nova Scotia, and find that there is little or no correlation possible between their growth-ring diagrams. From this circumstance, since such correlation is simple when dealing with contemporary trees, they conclude that the submergence was a slow one, considerable periods of time elapsing between the killing of one tree and the next.

J.G.D.C.

Pollen analysis: an outline of the problems and potentialities of the Method, by H. Godwin. Part I: Technique and interpretation. Part II: General applications. *New Phytologist*, 1934, XXXII, 278-305, 325-58.

These articles are important for archaeologists in that they describe the difficulties and complexities of the pollen-analytical method of reconstituting forest history.

J.G.D.C.

Zur Bearbeitung der vorgeschichtlichen und neueren kleinen Rundbauten der Pyrenäenhalbinsel: separata da 'Homenagem a Martins Sarmento, by Frhr. von Richtogen.

An interesting illustrated account of modern primitive corbel-roofed 'beehive' huts: compare M. Fournier's monograph, reviewed in *ANTIQUITY* 1934, VIII, 117. Of interest to British and Irish archaeologists, because these existing buildings are similar to some prehistoric ones in these islands.

Ancient ponds near Patcham, by H. S. Toms. *Sussex County Magazine*, August 1934, VIII, 486-90.

Ancient Ponds near Falmer: Broad Shackles and Buckland Bank, by H. S. Toms. *Sussex County Magazine*, September 1934, VIII, 546-50.

Mr Toms proves the pre-Roman antiquity of some of our downland ponds in two interesting articles illustrated by his own beautifully drawn and original plans.

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Some aspects of Nandi stock-raising, by G. W. B. Huntingford. *Journ. E. Africa & Uganda Nat. Hist. Soc.*, no. 49-50, pp. 250-62.

Of interest to prehistorians who deal with pastoral peoples.

Equatorial Palaeolithic Varves in East Africa, by Gerard De Geer. *Geografiska Annaler*, 1934, pp. 76-96 and 2 ph.

After some preliminary remarks which do not concern East Africa, the author refers us to the important work of Erik Nilsson on the terminal moraines of Kilimandjaro, Kenya, Elgon and Ruwenzori, and on East African lake-deposits (*Geogr. Ann.* 1931, XIII, pp. 241-348; Stockholm, 1932). Nilsson considered also that he had found annual varves, with the reservation that each year may have had two rainy periods and two dry seasons, instead of half that number of annual ice-meltings in temperate and circumpolar regions. Layers of volcanic ash appear at intervals in the laminated deposits. De Geer considers that he has identified the records of these varves with sections of his Swedish time-scale, and of Halicki's series in Poland. Such strange coincidences of teleconnection cause one to think furiously.

The use of the names of constellations, and other terms to describe portions of graphs is perhaps a little confusing. The biennial maxima of the last thousand years before the end of the Ice Age in Sweden are included.

Most geologists are hard to convince, and there are some who still accept Penck's ice ages in the Alps but not in East Africa, and De Geer's geochronology in Sweden but not its teleconnection with North America and the tropics. Archaeologists might do well to remember this. K.S.S.

Geology and Geochronology, by Gerard De Geer. *Geografiska Annaler*, 1934, pp. 1-52 and 2 ph.

In this paper Professor Gerard De Geer gives us a detailed statement of his method of linking up annual-banded glacial clays (varves) on a world-wide basis (teleconnection), after explaining carefully his method of close connections (geochronology by varve-connections) in Sweden. It is a very welcome statement and should be studied from beginning to end by those who attempt long-range correlation of Quaternary climatic oscillations. They will learn much of the difficulties when they realize that De Geer and his very able pupil Ernst Antevs, now resident in North America, have come to diverging views on methods, interpretation, and values, which are discussed at length in the paper.

There is a valuable supplement concerning sources of error—twelve sources of physical error, six of observation—which should be sufficient warning to intending teleconnectionists to tread carefully. K.S.S.

The Role of the Desert, by A. J. McInerny. *Studies Publications*, Channing Press, 8 Bream's Buildings, London, 1933. pp. 43, with 10 illus. and maps. 4s 6d.

The thesis of this book is that civilizations owe their creation to men invigorated and disinfected by the pure 'electrical' air and ozone of deserts—in fact an enlarged version of an old fashioned medical theory. The author's competence in archaeology—and English—may be illustrated by the following *literal* transcription of a representative passage:



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'The formation of an advanced civilization in the Sahara and over North Africa created a barrier to the further penetration northwards of palaeolithic man of the Neanderthal type. Furthermore flint implements of the old palaeolithic period have been discovered in Gafsa (Tunisia) and in Constantine (Algeria), which, according to Chantre and other authorities are identical with the Chellean, Acheulian and Mousterian periods of Europe'. V.G.C.

Ancient Latin Cities of the Hills and the Plains, a Study of the Evolution of Settlements in Ancient Italy, by G. Säfslund. *Opuscula Archaeologica*, vol. 1, fasc. 1, pp. 64-86; edidit Institutum Romanum Regni Sueciae.

Säfslund starts from the general position that city-life in Central Italy has moved from the hills to the plains and *vice versa* according to shifting economic and social conditions. The *oppida* of the Early Iron Age peoples were hill-settlements. After the 2nd Punic War, the Romans replaced them, according to Etrusco-Roman methods of town-planning, by cities built on the plains (it is shown by an examination of certain 'Latin' colonies that, before the 2nd Punic War, Rome contented herself with strengthening existing *oppida*, without moving them). In the Middle Ages there was a movement from the plains back to the hills, while at the present day the reverse tendency is again at work. R.C.C.

Place-name study in the United States of America: see references given in the *Geographical Review*, October 1934, pp. 659-60.

The early casting of iron: a stage in iron age civilization, by Thomas T. Read. *Geographical Review*, October 1934, pp. 544-54.

Drift Iron: a fortuitous factor in primitive culture, by T. A. Rickard. *Ib. id.* 525-43.

The Awash river and the Aussa Sultanate, by Wilfred Thesiger. *Geographical Journal*, January 1935, LXXXV, 1-23.

Contains a brief description, with illustrations, of modern types of grave and cenotaph, in stone and wood, in Eastern Abyssinia.

An evaluation of recent Nebraska finds sometimes attributed to the pleistocene, by Earl H. Bell and William van Royen. *The Wisconsin Archeologist*, April 1934, XIII, 49-70.

A severely critical and scientific examination of certain finds. The result is inconclusive, but leaves the door still open for pleistocene man in North America.

The beginnings of North and South in China, by Carl Whiting Bishop. *Pacific Affairs*, 1934, VII, 297-325. (Institute of Pacific Relations, 129 East 52nd street, New York).

The ancient highways and tracks of Worcestershire and the Middle Severn basin, by G. B. Grundy. *Arch. Journal*, 1934, XCI, 66-96.

Facts about the old road-system of this area, derived mainly from pre-Conquest documents.

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A ruined Nestorian city in Inner Mongolia, by Owen Lattimore. *Geogr. Journ.*, December 1934, LXXXIV, 481-97.

Contains photographic illustrations of stone slabs with Nestorian crosses carved on them.

Ancient Civilizations of the Santa Valley and Chavin, by Cornelius Van S. Roosevelt. *Geographical Review*, January 1935, pp. 21-42.

The Great Wall of Peru, discovered by aeroplane in 1931, examined on the ground. Other most interesting and well preserved remains found and photographed. We wish Dr Tello could persuade someone to make an air-survey of ancient remains in Peru; it is a promising land for this branch of research.

Ancestors of the Tuaregs, by E. F. Gautier. *Geogr. Review*, January 1935, 12-20.

Illustrations of rock-paintings, some of men in chariots claimed to be the Garamantes, whose capital Garama is now Jerma, in Fezzan.

The Scope of Economic History, by J. F. Rees. *History*, December 1934, XIX, 221-33.

This is a very readable review of six books, all of which seem, from the description given, to be most interesting. The reviewer claims that Mr Nef, dealing with the rise of the coal industry, 'has made it necessary to reconsider the economic history of the later sixteenth and the seventeenth centuries in the light of his thesis'.

Wat's Dyke: a field survey, by Cyril Fox. *Arch. Cambrensis*, December 1934, 205-78.

This concluded Sir Cyril Fox's survey of Offa's Dyke and Wat's Dyke, a fine example of a programme of field-work planned to cover a span of years and actually carried out in ten.

## BOOKS RECEIVED

TRANSACTIONS OF THE LANCASHIRE AND CHESHIRE ANTIQUARIAN SOCIETY, 1932.

GUIDE TO THE BUDDHIST RUINS OF SARNATH, by Daya Ram Sahni, Director General of Archaeology in India; 5th edn, 1933. 10 annas (1 shilling), Delhi.

THE DOWNFALL OF THREE DYNASTIES, by Count Egon Corti. *Methuen*, 1934. 16s.

TIRUPARUTTİKUNRAM AND ITS TEMPLES. *Bulletin of the Madras Govt. Museum*, N.S. General Section, vol. 1, part 3, 1934. 11 rupees 4 annas.

NEW HISTORY OF RYE, by Leopold A. Vidler. *Cambridge, Hove*, 1934. 12s 6d.

MATTER, MYTH AND SPIRIT, by Dorothea Chaplin. *Simpkin Marshall*, 1935. 8s 6d.

CREATION AND EVOLUTION IN PRIMITIVE COSMOGENIES, by Sir J. G. Frazer. *Macmillan*, 1935, 8s 6d.



## Reviews

THE ORIENTAL INSTITUTE. By JAMES HENRY BREASTED. Vol. XII of 'The University of Chicago Survey'. Chicago: University of Chicago Press, 1933. pp. 455, with 207 illustrations and map of operations. Price not stated.

This publication, the 12th and final volume of the series comprising the University of Chicago Survey, is in every respect as complete and satisfying a piece of work as we have come to expect from Professor Breasted.

The Oriental Institute is a truly amazing organization, since it has advanced from nothing to its present impressive dimensions entirely during the post-war years, and very largely by the tireless efforts of Professor Breasted himself, who has been the mainspring of the whole concern. Its present activities cover the following areas:—Persia (Persepolis), Iraq (Khorsabad, Tell Asmar and Khafaje), Syria (Kalneh), Anatolia (Alishar), Palestine (Megiddo), Egypt (epigraphic, architectural, and tomb paintings, Luxor; mastaba paintings, Sakkara; temple reliefs, Abydos; coffin texts, Cairo; and a Prehistoric survey). In addition to these there are several preliminary excavations going on at various sites, and there is the compilation of an Assyrian dictionary, which is alone a task of great magnitude.

Even Professor Breasted himself describes this as an ambitious programme which should be carried on for centuries, but it is none too ambitious for the tremendous importance of the task undertaken. Although excavations have been carried on in Egypt for the past century, it is only now that the rich area of the 'intercontinental bridge' from the Nile to the Euphrates is being adequately dealt with. As Professor Breasted states, this is undoubtedly the cradle of the culture of the Old World, and the results to be expected are of inestimable significance.

When we consider the formerly accepted methods of conducting expeditions in Egypt and the Near East—methods tied hand and foot by an old-fashioned outlook on archaeology—it is a matter for profound gratitude that the Oriental Institute has arisen to set a standard of work which has not hitherto been attainable. It is not suggested that the Oriental Institute alone has escaped from the thralldom of obsolete methods and procedure, but it is beyond question the first to be able to carry out a programme on a proper scale unhampered by lack of funds.

This question of funds has always been the bugbear of archaeological field-work; and is to a great extent unavoidable since the average important subscriber has usually little or no archaeological knowledge, and his interest requires the stimulation of objects found and displayed. More often than not these latter are of greater scientific than general value, and frequently a scientifically successful expedition may have no objects whatever to display. For instance, the first season's work which the present writer carried out on the Middle-Kingdom Egyptian fortress of Mirgissa in the Sudan entailed months of work with hundreds of natives shifting thousands of tons of sand, merely to uncover a small portion of the encircling ditch and ramparts, without the discovery of a single object. Subscribers often fail to see the point of such labour.

The Oriental Institute has been more than ordinarily fortunate in the broad outlook of its 'fairy godmothers'. Mr John D. Rockefeller, jr., has been from the beginning



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of the Institute's development the principal pillar of its financial structure. The generosity of the Institute's supporters has enabled it to employ the best men available in each branch of the work, and to skim the cream from most of the dormant or defunct expeditions which have suffered from lack of funds, causing restriction and curtailment of activities; and has also enabled it to build throughout the wide field of its activities what are really the first adequate quarters provided for any field-expedition anywhere. The workers can thus live in reasonable comfort in unpleasant climates, such as are found in most of the Near East, and, as a natural result, can work at the maximum efficiency.

The rapid growth and wide ramifications of the Oriental Institute have naturally stirred up against it a certain amount of adverse criticism, and not a little jealousy—that curse of archaeological as of much other scientific work. Particularly was this the case in Egypt during the first few years, and much of it can be traced to the Institute's offer of a new National Museum to the Government of Egypt. These negotiations, unfortunately, fell through.

Most of the general criticisms of the Institute, however, can be traced to motives which scarcely deserve serious attention, and reflect little credit on their originators. One is that the Institute's buildings are on too grand a scale, and that money thus spent might have gone most profitably to archaeological work. What such critics need to remember is the scale of the whole undertaking, and that the day of the small seasonal excavations, here today and somewhere else tomorrow, where the staffs operate under conditions of discomfort and poor food (to the detriment of their work) is drawing to a close. Not only are Departments of Antiquities everywhere insisting on a standard of excavation and record which renders such concerns impossible, but the standard demanded by new students of archaeology and by the world at large is vastly higher than it was even a few years ago. The Oriental Institute has built differently, and the quality of the results even at this early point in its progress is sufficient reward for the 'experiment'.

Another peg for criticisms is the question of the suitability or otherwise of the various expedition buildings for the purposes they are intended to serve. From personal acquaintance with the different Luxor houses, and with that at Bedrashein (for Sakkara), there would seem to be no grounds whatever for any such criticism; and the descriptions and illustrations of other buildings in Professor Breasted's book show the care with which each house has been individually planned for its special purpose. There were in the beginning certain inevitable 'snags', owing to the architects having to plan for conditions climatic, and otherwise, which were strange to them. But the experience gained from the first buildings has been taken full advantage of to avoid such minor unsuitabilities as were inevitable at first. The new Headquarters at Luxor, the Expedition Houses at Megiddo, Alishar, Chatal Hüyük, Tell Asmar, and the American Headquarters at Chicago are all sufficiently clearly illustrated and described in the book to show this.

Among the many familiar names of workers whose reputations were already made in Egyptology before being brought into the service of the Oriental Institute, one of the most welcome is that of Amice Calverley, of whom the Professor says:—'It is difficult to say too much in praise of the magnificent work . . . No better draftsmanship has ever been available in the service of archaeology, . . . the writer . . . feels the question may be fairly raised whether any drawing as good as this has ever before been done in Egypt'. Those who saw Miss Calverley's first work in Egypt, before she joined the Oriental Institute, and when she was with Professor Junker and others at Giza, said very much the same thing.

Certainly the Oriental Institute has taken full advantage of its unique opportunities



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for acquiring the best talent available in each branch of the work undertaken, and there can be nothing but praise in this respect.

The Director of the University Survey, in a foreword to the volume, voices the hope 'that this extremely interesting document may induce other large research enterprises to make corresponding analyses of their activities', and one can heartily endorse that hope.

NOEL F. WHEELER.

**EXPLORATIONS IN SIND.** By N. G. MAJUMDAR. *Memoir 48 of the Archaeological Survey of India*, 1934. pp. III, 172 and 46 plates, including map of Sind. 27s 6d.

This is an important work dealing with the results of survey in Sind for the Archaeological Department of the Government of India during the winter seasons 1927-8, 1929-30 and 1930-1.

The primary object of this survey was to ascertain the nature and probable period of the many deserted mounds that were known to exist in Sind, and Mr Majumdar has proved beyond any doubt that he was eminently suited for this very onerous undertaking. This account of his investigations will be indispensable to every student of ancient pottery.

In his first short season the author made a thorough examination of the mounds of Jhukar, 16 miles north of Mohenjo-daro, one of which is 63 feet high, and found there two kinds of prehistoric ware, the earlier of which is well represented at Mohenjo-daro. The upper strata of the mounds yielded Indo-Sassanian pottery and other objects.

In the season 1929-30 he traversed some 2000 miles of the Indus Delta country and examined over a hundred ancient sites.

A further survey in 1930-1 took him northwards along the Indus where he photographed and examined a considerable number of hitherto unexplored sites. A third journey was contemplated for 1931-2 to test the ground in eastern Sind, a great deal of which is desert, but this had to be postponed owing to the financial depression that involved India equally with other countries.

One of the most important results of this survey was the finding, first at Amri in Lower Sind and later at other sites, of a culture that evidently antedates that of Mohenjo-daro and Harappā; the pottery produced by this people was entirely dissimilar to the black-on-red ware that is so common in the latter two cities. It is wheel-made, thin, and decorated with geometric designs in two colours on a pink or cream slip or wash, and it is identical with some of the wares unearthed by Sir Aurel Stein in southern Balūchistān. It was found in a dark-earth stratum beneath deposits containing the black-on-red ware of the Indus Valley cities. The people who made this 'bichrome pottery', as Mr Majumdar terms it, but which I should prefer to describe as 'polychrome', including the slip on which the colours were applied, lived in stone-built habitations and evidently had a quite advanced civilization. But apart from the pottery that they made, we at present know little about them; few other objects were found, since the nature and terms of his survey prevented Mr Majumdar from making extended investigations on these particular sites. It is hoped, however, that more extended excavations will shortly be made, the results of which should be of the utmost value in the interpretation of the prehistory of India.

As is well known, both Mohenjo-daro and Harappā, situated as they are in the alluvial plain of the river, are built entirely of burnt brick; but that other and more convenient materials were on occasion used by the people of the same civilization is proved by two sites on rocky ground close to the Khirthar Range being constructed largely of stone.



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At a site named Kohtras Buthi, on the top of a hill some 48 miles northwest of Hyderabad, stone ramparts and bastions were built of large boulders without the use of mortar; and as far as could be seen they enclose an extensive quadrangle filled with the stone foundations of houses, whose superstructures were made of some other material, possibly unburnt brick or mud and wattle. The few sherds found in and around this fortress, and a couple of well-preserved clay tumblers prove it to be the work of the same people who built Mohenjo-daro. Further north at Ali Murad, some 65 miles southwest of Mohenjo-daro, a long recessed stone rampart constructed of roughly dressed blocks, averaging 2 by 1 by 1 feet, was traced a distance of some 170 feet; it still stood some 5 feet high in places. This wall also apparently enclosed a settlement and it had at least one well defined entrance. The painted pottery found at Ali Murad belongs to the same period as that of Kohtras.

No fortifications have as yet been found at Harappā and the question of a city wall at Mohenjo-daro had unfortunately to be left unsettled; but the existence of two hill-forts, one comparatively close to the latter city, leads one to suspect that the cities of the plain were also fortified. Ali Murad commands the Phusi Pass which caravans still use, and it therefore required strong protection from raids by the tribes of Balūchistān, which people, there is strong reason to believe, were a constant menace to Mohenjo-daro in the later stages of its occupation.

It is northwards from Hyderabad that the prehistoric sites of Sind exist in profusion; but well south of this modern town three prehistoric settlements were identified by Mr Majumdar, one of which, at Tharro immediately below Tatta, is studded with innumerable flint flakes and is thought to have been a flint-knapping station. This definitely shows this southern part of Sind to have been dry land in prehistoric times. By far the greater number of sites, however, are situated between the present bed of the Indus and Balūchistān in a chain some 180 miles long; but the eastern side of Sind, now for the most part stark desert, may produce more sites when it has been examined. One very large and particularly important site on the eastern bank of the Indus almost opposite Amri is Chānhu-daro, where in the short time that he was able to devote to its examination, Mr Majumdar unearthed a considerable amount of material. Seals like those from Mohenjo-daro, painted wares, pottery figurines, one in particular of a type quite new to us, and clay animals, as well as an etched carnelian bead, convinced him that this large site was inhabited by people of the same culture as Mohenjo-daro. The exceptionally fine pottery found here is, however, of a much better quality than any from Mohenjo-daro; and this fact has led Mr Majumdar to express the opinion, which the reviewer shares, that Chānhu-daro is of earlier date than the former city though the culture is the same. The two large painted storage jars from this site that he illustrates were found not far below the surface, and in the profusion of their decoration and its careful delineation they are far superior to anything that has been found in the northern city.

In a chapter entitled 'General Observations', Mr Majumdar sums up the results of his work. He points out that none of the prehistoric sites were reoccupied after their desertion, and that they are far away from the present habitations of man. With Sir Aurel Stein he is inclined to agree that the cause of their desertion was desiccation, and he suggests that the inhabitants moved gradually to the eastern and more humid parts of India, where, the reviewer would like to add, they were probably finally absorbed by the existing people, and lost their identity in the enervating climate.

Several prehistoric sites round Lake Manchhar have yielded painted pottery and chert flakes which were found embedded in the water-logged silt, and Mr Majumdar's



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observations have led him to believe that lake-dwellings existed in prehistoric times round this large sheet of water.

The author's analysis of the various kinds of pottery found by him at the sites that he visited can be usefully summarized as follows:—

- (A) The earliest: three-coloured, thin, unpolished ware of pinkish clay, with geometric patterns. Found for the first time in Sind at Amri, and subsequently at a number of sites in Western Sind. The shapes of these vessels are not unlike some that have been found at Nal, of which they could well be the prototypes. The colours used are black, chocolate or a reddish-brown, sometimes on a slip or wash of buff or light red colour.
- (B) Well-baked thick ware; with designs that are rarely wholly geometric painted in black on a well polished, red slip. Particularly fine examples were found at Chānhu-daro, and a later and more decadent variety occurs at Mohenjo-daro, where unpainted wares became more common than the painted.
- (C) Thin ware; decorated in black or chocolate on a light red or buff slip, with in some cases a reddish-brown band at the neck. Plants or flowers, sometimes very stylized, are common motifs. Mr Majumdar is uncertain to what period to assign this particular ware, and he suggests that it may be late prehistoric, like some of the pottery of Jhukar and Lohumjo-daro.
- (D) Black ware; with incised geometric patterns, found at Jhangar near Lake Manchhar associated with a little painted pottery. This black ware is compared by Mr Majumdar with some of Early Iron age found in the Madras Presidency. Black ware, or rather a grey ware with a black slip, is fairly common at Mohenjo-daro; but only one specimen has been found that was incised and this came from a high level.

Mr Majumdar is unable to trace the development of the second class of pottery from the first, and he rightly, it seems to me, regards the two as the products of entirely different cultures. Class B, it appears, extended over a considerable range of time, at first in universal use and then gradually succumbing before the plain, unpainted ware. It would seem, therefore, that the early polychrome and more simple ware (for we must also take shape into consideration) was made by a people who occupied Balūchistān and Sind, and possibly other parts of India, in very early times, but who, it would appear, together with their wares gradually gave place to another culture—it may be, a more highly civilized people—whose identity as well as that of the first group has now been lost. Whence did the people who made the class B pottery come? Mr Majumdar suggests that as the ibex appears on the black and red pottery of the hilly region of western Sind, principally on the wares from Karchat, it was from the highlands to the west, a conclusion that has already been arrived at by others on other grounds.

A few misprints in this most useful book by no means detract from its importance, and the author is to be congratulated both on the success of his explorations in the field and on having presented his material in a very able manner.

ERNEST MACKAY.

**THE ATHENIAN ASSESSMENT OF 425 B.C.** By BENJAMIN DEAN MERITT and ALLEN BROWN WEST. (*University of Michigan Studies: Humanistic Series*, vol. XXXIII.) Ann Arbor: University of Michigan Press, 1934. pp. xiv, 112, with 2 plates and 17 figs. \$2.50.

No Greek inscription, perhaps, has evoked a keener interest, or confronted students with more serious problems, than that which records the decree of 425 B.C., ordering a re-assessment of the Athenian Empire, together with the appended schedule containing



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a list of its component members and the sum at which each was assessed. The document was re-edited in 1924 by Freiherr Hiller von Gaertringen (*IG.* 1<sup>2</sup>, 63) and in January 1933 by the present reviewer (*Selection of Greek Historical Inscriptions*, no. 66). I then acknowledged gratefully my indebtedness to Professors Meritt and West, not only for the help received from their published works, but for their generosity in keeping me abreast of the results of their minute examination of the stele and their successive attempts to reconstruct the text down to the late summer of 1932, and I expressed the hope that they would themselves publish the text with an adequate commentary. That hope is now fulfilled and in the book before us we have an edition which must unhesitatingly be preferred to all its predecessors. Not that the last word has now been spoken: it is not the task of a work such as this to cry a permanent halt, but rather to consolidate the position already won and so prepare the way for a further advance. Nevertheless, unless and until new fragments of this inscription, or of the duplicate erected in the Council House (line 24), come to light, the present volume must remain the basis of all serious study of the important document with which it deals.

Chapter I contains a detailed inventory of the 43 inscribed fragments of marble (of which 25 are illustrated<sup>1</sup> by excellent photographs) which have survived from the massive stele.<sup>2</sup> A full bibliography of each is given, with notes on its contents and contacts. The editors have wisely decided to number the fragments afresh, 'following approximately the order in which they should appear in the text of the inscription' (p. 1), and have thus freed us from the awkward references inherited from the first edition of *IG.* 1.

In chapter II the materials thus collected, which include three small fragments (4, 17, 18) hitherto unplaced, are assigned to their positions in the stele. The resultant reconstruction, carried out in 1933 in the Epigraphical Museum at Athens, is shown in a diagram (fig. 16) and in a photograph (fig. 17), which vividly suggest the extreme difficulty of the undertaking essayed by a long succession of scholars (see the bibliography on p. 43) and carried so much nearer to its goal by Professors Meritt and West, aided by the 'helpful criticism' of Professor W. Kolbe and Mr H. T. Wade-Gery.

Chapter III comprises a restored text of the decrees and of the heading prefixed to the appended assessment-list, together with a translation and a full commentary. Despite the fragmentary nature of the stele, the reconstruction offered is so thoroughgoing that nothing, with the exception of a few proper names, is left unguessed at. That the text so won reproduces exactly the words of the original the editors do not assert. Notwithstanding the invaluable aid derived from the *stoichedon* arrangement of the letters and the fact that the stone is 'one of the most carefully inscribed of all the documents of the fifth century' (p. 54), the task of restoring the inscription presents well-nigh insuperable difficulties due to the unique nature of its content and its consequent failure to conform to familiar formulae. The modesty of the editors' claim that 'the restorations offered... give at least the general sense of the inscription' (p. v) disarms criticism of particular words and phrases and is as creditable to them as is the meticulous care and unflagging industry displayed in every part of their work.

<sup>1</sup> One fragment (6) is lost: the remaining seventeen (21-31, 35, 39-43) were illustrated in West's article in *Metropolitan Museum Studies*, III, 174 ff. As that article is not widely accessible in this country, I cannot but regret that the photographs there published have not been reproduced in the book before us: they would have materially increased its usefulness without adding unduly to its bulk or cost.

<sup>2</sup> These do not include *IG.* 1<sup>2</sup>, 151, which, to judge by the character of the script, may possibly come from the latter part of the inscription.



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In the fourth and final chapter the authors discuss the assessment-list which follows the decrees, arranged in four columns, each of at least 113, and more probably of about 120, lines in length, inclusive of some lacunae left uninscribed between one district and the next. To the four districts familiar to us from the quota-lists after 440 B.C. two further groups of cities are added—one consisting of thirteen 'Actaeon' cities, added to the Empire after the failure of the Lesbian Revolt, and one of a considerably larger number of Pontic cities. Finally, an attempt is made to determine a 'minimum' and a 'probable' figure for the total assessment of each of these six areas and so for that of the whole Empire, resulting in the unhesitating acceptance of the view—strongly advocated by Kolbe, but hitherto combated by the authors in common with the great majority of scholars—that the grand total amounted to a sum of 1460–1500 talents. Three invaluable indexes, one of which registers all the known Athenian tributaries<sup>3</sup> and another the significant Greek words,<sup>4</sup> and two plates bring the volume to a close.

The beauty of the printing, the scrupulous care with which the proofs have been read<sup>5</sup> and, above all, the lucidity, sobriety and cogency of the argument make it a sheer delight to read the book. Where so much is given, it may seem ungracious to ask for more: yet a table of concordance between the old and the new numeration of the fragments would save the student appreciable time and trouble, while some reference should surely have been made in chapter 1 to the provenance of the fragments. With an appetite whetted rather than satisfied we look to the authors to give us a comprehensive study of fifth-century Athenian finance, among the materials for which the present document will take an important place. In that study they will doubtless deal with questions raised, but not answered, in the book before us, as for instance, how they interpret the fragments left to *IG. I<sup>2</sup> 64* (see p. 19), which they still maintain (in opposition to Kolbe) is not a second copy of the same document as *IG. I<sup>2</sup> 63* (pp. 24 f. 81 f. 84).<sup>6</sup>

MARCUS N. TOD.

**PARTHIAN POTTERY FROM SELEUCIA ON THE TIGRIS.** By NEILSON C. DEBEVOISE. (University of Michigan Studies, Humanistic Series, vol. XXXII). Ann Arbor: University of Michigan Press, 1934. pp. 132, 16 plates and 423 figs. \$3.

In all the great archaeological expeditions in Mesopotamia Parthian pottery has been excavated, but these finds from Nineveh, Ctesiphon, Babylon, Nippur and many other sites were unimportant compared with the extraordinary objects from earlier levels. Hence they do not play a prominent part in the archaeological reports, so that our knowledge of the Parthian wares has been on the whole rather vague. The campaigns at Seleucia from 1927 to 1932 undertaken on behalf of the American School of Oriental Research in Baghdad under the auspices of the University of Michigan and supported by the Museums of Toledo and Cleveland have, however, quite changed the situation. An enormous amount of well-preserved pottery was excavated—in two seasons alone nearly 1600 pieces—and it is excellently described in Mr Debevoise's well-illustrated book.

<sup>3</sup> On p. 93 we must read [Ἀλονόριος] and on p. 97 Ναξάται παρὰ Μύριναν and, probably, Οὐθώριαι (cf. Θέστωρος, Πίλωρος, both in Thrace): What of the [Μυ]γισσῆς of *SEG. v, 4. IV. 19*?

<sup>4</sup> For μηδεμία (pp. 45, 104) read μηδεμιά, for πρήθουρα (p. 107) πλῆθουρα, for πορευθήσονται (p. 106) πορευθήσονται (as on p. 105).

<sup>5</sup> On p. 19 'L. Franz' should be 'J. Franz', and 34 should be added to the fragments removed from *IG. I<sup>2</sup> 64*; on p. 43 'O. Müller' should be 'I. von Müller'.

<sup>6</sup> Why should 8 rather than 13 talents be restored in *IG. I<sup>2</sup> 64* as the assessment of Antandrus (p. 81)?



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These finds from Seleucia are especially significant scientifically, because, despite the fact that the Parthian period lasted here only about 340 years (141 B.C.-c. A.D. 200), they were found in three different levels, thus providing accurate dating for nearly all pieces. This alone will make Mr Debevoise's book indispensable for all further excavators in Mesopotamia, for with its help other Parthian potteries can now be readily dated. Two important circumstances have, however, to be considered, while making use of the material: the geographical situation of Seleucia and its strong Hellenistic heritage make it representative only of the westernmost part of the Parthian Empire; and the excavations have proven that the town steadily diminished in its importance on account of the strong competition of the newly founded Ctesiphon, a decline only too visible in the potter's craft, as the later wares are all far more carelessly manufactured and glazed.

Mr Debevoise establishes and gives a general discussion of 417 types, all reproduced in drawings; he then considers the character of the glazes, supporting his statements by several quantitative and qualitative analyses; and adds finally an exhaustive catalogue of all types. From an artistic point of view the pottery is on the whole rather poor, only occasionally pleasing by graceful shape, a fine profile or, more rarely, by some artistic decoration. The pottery is, however, very often impressive by virtue of the sturdy shapes, expressing a strong and effective character. The glazed wares have on the whole a simpler ornamentation than the unglazed ones, especially the lamps, as the thick glaze would conceal finer or more intricate patterns. The material discussed contains many odd types, such as toy fountains with turtles and fish; saving-banks with slots to insert coins and an apparatus apparently used to soften bullae to receive seal impressions. A kind of dark lantern (similar to nos. 350 and 351) had already been published by J. P. Peters, who found it in Babylonian graves at Nippur (*Nippur II*, pl. VI, 61), while a drinking vessel, a kind of amphora with a spout at the lower end (nos. 176 and 177) seems to be the simple forerunner of the grandiose rhyton in the Kelekian Collection in the Victoria and Albert Museum, which is, however, wrongly repaired;\* the decoration over the spout on these Seleucia vessels represents bovine horns, being the last relics of an entire bovine head such as is found on similar Parthian drinking vessels recovered in Persia and Mesopotamia. One may regret that Mr Debevoise does not include in his survey of the Seleucia pottery the most remarkable achievement of the potter's craft, the coffins. Examples both of the bath tub and the slipper-shaped sarcophagi were excavated. The fact that the latter were found only after the third level (141 B.C.-A.D. 43) had been reached, seem to solve the difficult problem of their date. Unfortunately none of the stately amphorae, a common Parthian type and represented in nearly all the greater collections, have been found, so that their date is still uncertain. A very important decoration on these amphorae, little disks and knobs usually applied near the handle, could however be traced on several jugs and lamps of the second and first level (A.D. 43-200), thus proving that this device was in use in late Parthian times. Only very few examples of two-colour glazing were discovered, nearly all on lamps. This process is in these cases due not to an artistic, but to a practical intention, as the dipping of the nozzle of the lamp in a second glaze served to make it more resistant to the heat. It is an important fact that no lead glazes have been traced among the Parthian wares and that 'most of the glazes analysed were alkaline silicates with an oxide added for colour' (p. 30). The pottery was nearly all locally made—the expedition discovered also a

\* F. Sarre, *Die Kunst des alten Persiens*, Berlin, 1922, pl. 147.



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kiln—and the potters' repertoire included even such a purely Greek shape as an askos. The only imported wares were Roman Arretine pottery and a few vessels from the eastern part of the Parthian Empire. The catalogue contains also many pre-Parthian (Seleucid) potteries, thus enabling us to get a somewhat clearer conception of the historical development; it includes furthermore a graceful greenglazed bowl, whose supposed Sassanian origin is well proven by a very similar piece found in the Sassanian level at Kish and now in the Field Museum, Chicago (Nippur, K-1758).

R. ETTINGHAUSEN.

HOOTON PAGNELL: the agricultural evolution of a Yorkshire Village. By ARTHUR G. RUSTON and DENIS WITNEY. *Edward Arnold, 43 Maddox st., W.1, 1934. pp. viii, 459, and 12 plates, 58 text-figures. 25s.*

By those familiar with Dr Ruston's work over many years in the collection of records of all kinds illustrative of farming in Yorkshire, from pre-Domesday times to the present, this volume has been awaited with greatest interest. The authors in their collection of material have taken a particularly wide view of agriculture, recognizing it as the essential basis of the English village life and economy, affecting almost every aspect of the peoples' lives. By choosing as example a village with very complete records over a range of nearly a thousand years, it has been possible to trace not merely the economic history of the farming and systems of land tenure, but to give a strikingly complete picture of the village life and customs, and to relate this by quoted example to comparable and contrasted villages in many parts of Yorkshire and even in many parts of England.

The village of Hooton Pagnell is at present somewhat exceptional in being entirely the property of one landlord, with the exception of the few acres occupied by the North Eastern Railway. This single ownership has been arrived at comparatively recently, and has had the happy effect of bringing into a single muniment room, that of the Hall, a very complete set of deeds, surveys, leases, etc. These, along with the parish church chest, the churchwardens' accounts, and the tithe awards, have been supplemented by considerable study of existing traces of ancient fields, balks, field-names, etc, throughout the parish.

A detailed view of the parish of today is first given, with clear description of the farms, methods of farming, and the outstanding problems facing the farmers at present, leading to a set of pertinent questions which the rest of the book attempts to answer: *e.g.* How came the farms and farm lands into the possession of the present owners? In what way have the privileges and responsibilities of ownership been evolved?

The Domesday village is discussed after a very clear account of the making of the survey has been given, and like all the sections in the book this is accomplished largely by quotation and brief comment upon original documents. The extent and nature of the farm lands, meadows, woods, and common pasture are illustrated by surveys and in some cases plans, from the 12th to the 16th century, and the duties and services of the inhabitants of the manor made clear. The next section gives a survey of the open-fields prior to 1595, and then traces the consolidation of holdings by exchange of strips and later by enclosure, and the changes in the control of the Common and Commons Rights up to an award as recent as 1929. This is probably the most completely documented and detailed account of the whole process of evolution from the open-field strip holdings, to the present day farms, that has yet been published for any one area. The illustrative maps showing the evolution of individual farms and farm holdings are most valuable, and all interested in manorial history will find this chapter of constant



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value for reference. Systems and methods of farming introduce the question of ownership, and this is treated in detail, an interesting table being that showing owner, how acquired, relation to previous owner, etc., from the Saxon earl Edwin, to the last change of ownership in 1931.

A section of particular interest to the manorial historian deals with the changes in land tenure and position of tenants since those set out in the 10th century *Rectitudines*. A very detailed survey of the manor under the feudal system is succeeded by a study of the methods of commutation of services fully illustrated by abundant leases. The customs of the manor, tenancy by indenture, improvement leases, etc., will in their discussion be of use in interpreting the import of many old deeds and leases encountered by the local historian in any area. An interesting link with the 14th century is the ownership of the manor by the Louterells from 1215 to 1419, and the fact that the Geoffrey Louterell who commissioned the preparation of the Louterell Psalter was so closely connected with this manor. It may be that some of the illustrations of the Psalter are drawn from the very fields and farms studied.

The later part of the book deals with two questions of much present topical interest, the evolution of tenant-right and of glebe and tithe. These sections are particularly full for the more recent years, especially for the 19th century, and will take their place as a substantial contribution to the present problem of tithe adjustment.

The book is somewhat hard reading from the uniform method adopted of quotation of documents in sufficient detail and abundance to make very little comment necessary. This vastly increases its value as a safe work of reference, and makes all the more welcome the considered judgments given on present trends in farming practice and problems. The make up of the book is very pleasing, and the illustration very well chosen. Many gems from the Louterell Psalter and similar documents lighten the numerous farm and parish plans of various dates. The plates are good and well chosen. It is perhaps allowable to mention one error which has little bearing on the subject of the book: that is the titling of plate 5, where an excellent photo of the strip lynchets at Carperby is described as of Celtic lynchets! It would also have been a welcome addition if some details of the Hall, church, and glebe land, with a larger scale plan of the village, could have been added. Many readers in the North, however, will certainly wish to visit and explore the village further, and will supply this want themselves. A. RAISTRICK.

**L'INDUSTRIE DES ATELIERS A MAILLETS DE MURS.** (Extrait du Compte Rendu de la Xe session du Congrès Préhistorique de France, 1931, pp. 146-79, 17 plates and 18 figures). By A. VAYSON DE PRADENNE. *Le Mans*, 1934.

M. Vayson de Pradenne has again placed prehistorians in his debt by a masterly summary of an intricate problem. On this occasion he has turned his attention to the series of workshop-sites on an outcrop of chalky limestone, some 200 yards wide and between 3 and 4 miles long, near Murs in Vaucluse (Provence). The intense exploitation of this limited area was due to the excellent character of the flint and to the unique disintegrating structure of the rock. Characteristic of the site are numerous ovoid hammer-heads, many of which bear a central encircling groove for hafting, made from the local rose-coloured quartzite. Definite proof has been obtained of their use in extracting the flint by shattering the rock. Previous writers have focused attention on 'les maillets' to the exclusion of the associated flint-industries here studied by M. Vayson de Pradenne. From the debris of the flint-knapping workshops come 'tortoise cores', cores of Aurignacian type with two striking platforms and of Neolithic type with



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one, many of these being worked up as planes. Points and side-scrapers of Mousterian tradition, blades of Upper Palaeolithic facies, racloirs, tranchets and carinated fragments analogous to Campignian picks are common, but scrapers and blades trimmed on both faces, allied to Neolithic forms, predominate. This industry has been assigned to all periods from Mousterian to early metal age according to the relative significance attached to the flint artifacts and to the hammer-heads. The homogeneity of the industry is, however, beyond question. The writer concludes that the evidence of the quartzite hammers, the presence of a decadent flint industry and the vast scale of this industrial exploitation, all indicate a date at the dawn of the metal age—a fact of the utmost significance, delivering a 'coup-de-grace' to the arguments still advanced for dating based solely on typological affinities.

It should be added that this paper is well illustrated though the absence of a scale on the line drawings is annoying.

RAINBIRD CLARKE.

**DIE GERMANISCHEN VOLLGRIFFSCHWERTER DER JÜNGEREN BRONZE-ZEIT.** By ERNST SPROCKHOFF. *Römisch-germanische Forschungen*, bd. 9. Berlin: W. de Gruyter, 1934. pp. 142 with 44 plates. 24 marks.

This study of the 'full-hilted' swords of the later Bronze Age in central Europe follows the author's earlier work on the Germanic flange-hilted (tongue-grip) swords, reviewed in *ANTIQUITY*, September 1932, p. 376. Its format is the same, and it is similarly enriched by many excellent plates and distribution-maps. These show, with few exceptions, the entire distribution-area of each sword-type, and they mark a great advance in the study of these significant weapons and of the cultures they represent. The symbols are well displayed in red; and the only criticism that might be levelled at the maps is that they attempt to portray high ground by means of disconnected hachured masses in place of selected and clear, if generalized, contours.

Like other workers, Sprockhoff is almost exclusively concerned with the shape of the haft in his classification and typological analysis: he has little to say about the blades. He recognizes two swords which are strictly 'Nordic' in origin and distribution—the horn- and kidney-pommel varieties. Both were evolved in the Danish islands in Montelius IV; but the centre of gravity shifts in Montelius V to the middle Elbe region, the meeting-place of trade routes from Italy and the eastern Alps and from northwest Switzerland. Throughout the late Bronze Age close contacts with the south are evident in north Germany, and the 'Nordic' culture changes its character henceforth; it is transformed from an intense but narrowly circumscribed culture to one of varied content and wide-spreading influence. Of three well-known and characteristic swords dealt with—the Auvernier, Möriker and Antenna types—the Auvernier sword reveals by its distribution the close relations between Germany and Switzerland (where it was evolved), while the Antenna sword illustrates the connexion with Italy. The Italian specimens are all of the simple-spiral class, but that the type did not originate there is proved by the absence of transitional examples such as are found in central north Germany.

Sprockhoff follows earlier investigators in recognizing a Hungarian prototype for the Möriker sword. His map provides no justification for the alternative title, 'Rhône valley type': only 6 French examples are marked in the Rhône-Saône area, while there are over 100 listed from central and north-central Europe. He mildly questions the accepted belief in a Swiss origin, arguing that the type may well have originated in the middle Elbe. He is none the less impressed with the vigour of the west Alpine culture, and cites instances of the trade carried on with north-central Europe.



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A sixty-page catalogue of the five sword-types considered is a valuable feature of the volume; there are over 400 entries, and information is given as to the find-associations, literature and present whereabouts of each sword. Of rather special interest to English students is the fragment of the haft of an Auvergnier sword from the Croydon hoard (pl. 35, 6) which seems to have passed unobserved.

E. ESTYN EVANS.

STATUES-MENHIRS, STÈLES GRAVÉES, DALLES SCULPTÉES. By COMMANDANT OCTOBON. *Revue Anthropologique* 1931, pp. 299-579. (Sold as a separate publication in paper covers by M. Emile Nourry, 62 rue des Ecoles, Paris 5<sup>e</sup>; price, omitted from the title-page, 30 francs).<sup>1</sup>

Scattered over the Old World from Central Asia to Brittany and from Central Europe to Abyssinia and the Sudan, there may be found certain curiously carved upright stones. They are called 'statue-menhirs' by the French; an alternative name 'dolmen idols' is less satisfactory, because they are not invariably associated with 'dolmens', though often forming a unit in the megalithic culture-complex. Descriptions of these stones have appeared in countless books and learned transactions, and it was high time that someone should attempt to codify them. The first attempt is not an unqualified success, but it is much better than nothing at all, and it does provide a useful 'catalogue raisonné' of the French examples. If, as Professor Breuil says in the preface, the foreign portion of the monograph is less well documented, that is of course because a 'provincial' has not access to those foreign publications which are indispensable for such a task. This may account for, but does not excuse, the omission of any reference to the standard work on such subjects as the Easter Island statues (p. 507); it may also account for the amazing statement that this island has disappeared into the ocean! (disparu dans l'océan).

It would be wrong not to express a measure of qualified admiration for the industry which has produced this book; but with a little less elaboration it might have been much better and much more easy to use. Being primarily a book of reference (with the exception of section 4, the best and most readable portion) it should be easy to refer to, and that it emphatically is not. The catalogue (section 3: 'description sommaire') appears at first sight to be arranged in no sort of order, and in the absence of any index or paginated list of contents, one is lost. The descriptions of items are headed by the department, but they are not *grouped* under departments; and it is only after prolonged examination that one discovers (on p. 367) the heading 'IV<sup>e</sup> groupe', implying some system of classification. Further research reveals the fact that the items on pp. 310-67 are in fact classified according to the scheme set out on pp. 306-7, but apparently the author forgot to insert the headings until he came to group 4. There is a further complication in that the sub-headings given in the text are not verbally assimilated to those in the 'essai de classement'—the only place where the author's system is explained. The mania for classification is carried even to the extent of forming sub-groups; but, after working through the first six groups, the author seems to have given up the task.

The fact is that M. Octobon has never really thought out the first principles of classification. He rejects (on p. 305) the geographical method adopted by Déchelette and all modern students because it suggests 'groupements géographiques que la réalité ne sanctionne pas toujours rigoureusement'. We do not understand exactly what this means. But while rejecting the geographical method of classification, he is forced to

<sup>1</sup> We apologize for the long delay in publishing this review. The archaeologist to whom we sent the original review-copy died before he had written the review.



PLATE I



CHALK FIGURE, GROTTÉ DE COIZARD, MARNE. (See p. 111)  
*Ph. Armand Viret*

PLATE II



STELE OF KHEILA (See p. 121)



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adopt it in some degree—witness his two last groups (Groupe de Corse, 398-400, 486-7; groupe des statues étrangères, 400, 507-60). He seems to live in the old world of pure typology—a barren region long since evacuated by most students.

The illustrations are deplorably bad, but again are better than nothing. A study in black and white on p. 311 represents the well-known Coizard chalk-figure, though it looks like a lump of clay photographed in the dark. We reproduce here (PLATE I) M. Viré's admirable photograph of this important monument. (Perhaps some day the Marne rock-cut tombs will be planned and published!). We also reproduce (PLATE II) one of the interesting Moroccan stelas (one was published, equally obscurely, in the Report of the 15th Congress held in Portugal, 21-30 September 1930, also published by M. Nourry).<sup>2</sup> At the same time we may call attention to M. Vayson de Pradenne's article on 'La Stèle de l'Isle-sur-Sorgue (with its excellent photographs and drawings) published in the *Compte Rendu de la X<sup>e</sup> Session du Congrès préhistorique de France, 1931*.<sup>3</sup> To the list of foreign examples may be added a statue-menhir at Hadji Hara in Soviet Armenia, near Mount Ararat, with head and breasts. There is a photograph of this in the Museum at Erivan, together with one of a ruder example. Megalithic phalli in the same district are still resorted to by women for purposes of supposed impregnation.<sup>4</sup>

We do not wish to conclude this review without expressing gratitude to the author for his attempt to synthesize and codify a vast group of facts. We are all the richer for it. Whatever may be said of its short-comings we shall use it continually in any researches concerned with the ritual of the megalithic cult.

O.G.S.C.

ARCHAEOLOGIA ORIENTALIS, published by the Far Eastern Archaeological Society, Tokyo and Kyoto.

- I. P'I-TZU-WO: Prehistoric Sites by the River Pi-liu-ho, S. Manchuria. By KOSAKU HAMADA, 40 pp. in English and German, 73 § in Japanese, 69 pl. (10 coloured), 47 figures, 1929.
- II. MU-YANG-CH'ENG: Han and Pre-Han Sites at the Foot of Mt. Lao-t'ieh, S. Manchuria. By YOSHITO HARADA and K. KOMAI, 52 pp. in English, etc., 100 § in Japanese, 72 pl. (3 coloured), 45 figures, 1931.
- III. NAN-SHAN-LI: Report on the Excavations of the Brick-tombs of the Han Dynasty at the Foot of Mt. Lao-t'ieh. By SADAHIKO SHIMADA and K. HAMADA, 65 pp. in English, 108 § in Japanese, 55 pl. (3 coloured), 62 figures, 1933.

Kwan-to, the southern extremity of the Liao-tung peninsula, containing Port Arthur and Dairen, has played a great part in the history of the last forty years: Japan tried to obtain it after her victory over China in 1894, Russia leased it in 1898 and Japan finally got the lease in 1905. In Korea the Japanese have done much for archaeology by their excavations, particularly in the Lo-lang district in the northwest

<sup>2</sup> We are indebted to M. Benoit for these photographs, obtained through the kind offices of Mr W. J. Hemp, F.S.A.

<sup>3</sup> Imprimerie Ch. Monnoyer, 12 place des Jacobins, Le Mans. (We cannot give page references as all these separate unfortunately have separate pagination).

<sup>4</sup> An elaborate account of Abyssinian megaliths was published in 1931, the same year as this monograph (*Cinq années de recherches en Éthiopie*, par R. P. Azais et R. Chambard, 2 vols., Paris, 350 francs); and reference may also be made to M. H. Neuville's articles in *L'Anthropologie*, 1928, xxxviii, 255-88, 523-64; 1932, xlii, 497-523.



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(see ANTIQUITY, 1931, v, p. 521), and Kei-sho (Kyōng Dzhu) in the southeast, splendidly published by the Governor General's Administration.

They are doing similar work in Kwan-to, and it is being published by the recently founded Toa-kokogaku-kwai or Far Eastern Archaeological Society.

I. P'I-TZU-WO. Two sites near the coast at the extreme northeast of Kwan-to now left high and dry by the retirement of the sea, but once forming an island they have called Tan-t'o-tzu (single isle) and a little promontory named Kao-li-chai (Korean fort). The island was occupied first, mostly in neolithic times; artifacts chiefly of blue-grey slate, celts rather like the shoe-last, crescent-shaped knives with two holes, a dagger 16 in. long, a few beads and spindle-whorls and bracelets of shell: most of the types familiar from Torii's work. The chief interest is in the pottery: A<sup>1</sup>, monochrome ware, bowls, stands and globular pots; A<sup>2</sup>, the most easterly extension of painted pottery, less accomplished than Sha-kuo-t'un, but with an attractive step-pattern in red and yellow ochre and white slip; A<sup>3</sup>, black or buff ware more developed than A<sup>1</sup>, found in an earthen grave. All this fits on to the culture described by C. W. Bishop, ANTIQUITY, 1933, VII, p. 389.

At Kao-li-chai there was no painted pottery but a ware, a more advanced monochrome, rare at Tan-t'o-tzu, showing very well the prototypes of the hollow-footed *Li* tripod, and the two-storeyed steamer *Yen*, familiar in Chinese bronze. It was succeeded by c, of a fine grey-like late Chou or early Han ware.

There was a little bronze, and iron things overlapping with the bronze and stone. This stage is dated by knife, spade and Early Han coins. Occupation ended about 100 B.C. The inhabitants were perhaps a Tungus tribe (Torii), or more probably early Chinese beginning their expansion towards north Korea and their cultural influence on Korea and Japan (Hamada).

II. MU-YANG-CH'ENG (Pasture sheep fort) is an oblong earthwork on the west slope of Mt. Lao-t'ieh, the projection of the peninsula southwest of Port Arthur. It yielded a few Stone-Age objects and then seals, antefixes and coins pointing to its having been an administrative centre under the Han. The pottery dated from late Chou to Han and is important as being used ware, not funerary. The chief interest is in the tombs of the neighbourhood, a new type in which the coffin was packed outside with shells or stones, a child's burial in an arrangement of three pots of quite different wares, and most curious, the first known example of a *chi-chou* grave; by a practice ascribed to Chou times in the *Li-chi* the earth walls of the grave were turned into brick by lighting a fire in it. It yielded several bronzes, particularly a dagger handle of a queer shape like a laterally compressed mushroom with an inset like a dress-tie. This explains the hitherto unknown use of various bronze parts found in China and Japan and an excursus is devoted to these. An appendix describes an exactly similar earthwork in Shan-tung across the gulf.

III. NAN-SHAN-LI (S. Mountain to the left) is a western spur of Mt. Lao-t'ieh. Near it are many fine brick tombs of officials at Mu-yang-ch'eng in Han times. Each contained delightful model houses, stoves and animals in pottery, vases also and toilet boxes and some boxes in lacquer. These are interesting because such tombs have rarely been properly excavated in China, though they do not compare with the finds at Lo-lang. But the buildings are really the main thing, mostly two chambers with corbelled vaults, but one dug by Hamada in 1912 with no less than five. The bricks are patterned and even painted in bright colours—a novelty.

Each volume contains an appendix in German dealing fully with the human



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remains, which appear to be of the Chinese type, approximating as they get later to that of the present day. The three folios are magnificently produced: in NAN-SHAN-LI each tomb-group is figured as a whole and then the objects given separately on the plates, a very good arrangement. The maps are a little hard for the westerner as the lettering is all in Japanese, but the English summaries and descriptions of the plates are sufficient.

ELLIS H. MINNS.

**DAS URZEITLICHE BERGBAUGEBIET VON MÜHLBACH-BISCHOFSHOFEN.** By KARL ZSCHOCKE and ERNST PREUSCHEN. Published by the *Anthropologische Gesellschaft in Wien*, (1, Burgring 7), 1932. pp. ix, 287, 6 maps, 28 plates and 19 text-figs. Price not stated.

The district with which this imposing monograph deals comprises a strip of land in the Province of Salzburg stretching in an easterly direction between Mühlbach and Bischofshofen for a distance of about 20 kilometres, with a breadth of ten. The country is mountainous and highly mineralized. Evidences of early working are to be found in funnel-shaped depressions in the ground, trenches often extending to some hundreds of metres, ore-sorting stations and smelting sites.

Characteristic remains associated with the sites are comparatively few, as the sites of the homes of the miners still await discovery. The chronology of the remains is dealt with in a series of supplementary reports (pp. 153-85). No definite period can at present be assigned to them, but provisionally they may be said to belong to the early Bronze Age period and to have persisted to the end of that period, or even later. Following a general topographical description of the sites, we are given on pp. 25-39 a novel and striking disquisition on the application of fire-setting by the 'Old Men' to rock removal. It is surprising to hear that this ancient method can, under certain conditions, compete with the use of modern mining explosives. The ancient methods of breaking down the hard rock by fire will be best understood by referring to the four plates which follow maps I-VI. Here only an imperfect outline of the process can be given.

Fire-setting is effective only when the heat of the flame of the burning pile can be directed against the face of the rock to be removed. In the initial stage of mining carried out on a single floor, fire-setting produces little result. As the work is advanced and the ceiling heightened the floor is divided—one portion following the principal vein. The other is timbered and roofed for the protection of the workers and the waste rock is piled against it. Air now passes from the lower to the upper floor and fire-setting yields better results. On the lower floor drainage-water is collected and dammed. The water is partly utilized at a stamping-station placed on the further side of the dam. The remainder is carried by hand-labour to the surface. This thesis is based partly on observed facts and partly upon conjecture—but the writers seem confident of its substantial accuracy. For the present it seems advisable to regard their thesis as provisional only—for other solutions appear to be compatible with their data.

The following chapter deals with the prehistoric methods of copper extraction. A very clear and useful account is given of the rationale of copper smelting prior to the introduction of the electrolytic methods. The writers give their reasons for the non-acceptance of the single-smelting theory of Kyrle. According to their view the ore was twice roasted and twice smelted—the final product being marketed in round cakes



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and refined elsewhere. They also note that smelting was probably to some extent carried out in crucibles; for fragments of pottery found on the sites have been shown to contain appreciable quantities of copper slag, and the shape of these fragments militates against the idea that the vessels were made for domestic uses. The writers admit that their conclusions are based principally upon the result of slag analysis. If the four operations are accepted as the minimum number in theory the thesis may be accepted. It is, however, probable that in practice the old copper-smelters blundered as frequently as their successors, and that there was a good deal of unnecessary duplication of work. For instance, in 1582 Joachim Gaunse stated that he could 'make copper and copper stone by once roasting and once smelting' whereas the patentees of the Mines Royal could only do it with sixteen roastings. Gaunse proposed to lixiviate the roasted ore, as vitriol was then a profitable side-line—but he also alleged that the process of washing the ore 'causeth it sooner to melt'. Lixiviation would have assisted the operations of the Salzburg smelters, and as their piles of ore were stacked in the open probably much of the soluble metallic sulphates was removed by rainfall. Subsequent chapters deal with the location and duration of the mining settlements. The supplementary matter has already been referred to and it only remains for the present writer to congratulate Herren K. Zschocke and E. Preuschen upon the satisfactory treatment of their material. The work is well printed and illustrated. From a publisher's standpoint, however, it may be suggested that the address should be stated on the title-page, that the price of the publication should be given in slip form or otherwise, and that a full collation should be given of the preceding monographs whether in print or not. Further, a word of caution is advisable to intending purchasers of this work. Before attempting to master its contents they would do well to study with care the 'Verzeichnis und Erklärung der Fachaussdrücke', pp. 265-83.

E. W. HULME.

**MASADA: DIE BURG DES HERODES UND DIE RÖMISCHEN LAGER** mit einem Anhang: Beth-ter. By ADOLF SCHULTEN. (*Sonderdruck aus Zeitschrift des Deutschen Palästina-Vereins*; Band 56). Leipzig, J. C. Hinrich, 1933. pp. 184, 14 plates and 28 maps and plans. 15 marks.

The rock of Sebbe or Masada forms a high isolated plateau 2080 feet long by 1050 feet broad. From the top of its precipitous sides the Dead Sea can be seen lying far below on the east. On the northwest and southeast are the deep gorges of the Wad-el-Hâfâf and the Wad-es-Seyal, divided by a rocky saddle, and beyond lie the waterless crags of the Waste of Judea.

The story of the grim tragedy which will always be associated with the site is told by Josephus—the occupation of Masada by Eleazar and his sicarii, altogether some 960 men, women and children, the investment of the rock by the Roman troops under L. Flavius Silva, the building of the great mound on the west, the breach in the defences made by the battering ram, and the self-immolation of the garrison. There is probably no site that presents so clearly the *mise-en-scène* of a great tragedy of the ancient world.

On the summit of the plateau are the remains of the buildings that housed the garrison, the store houses, the cisterns, the defences. Down below the lines of the

\* Grant-Francis, *Smelting of Copper in the Swansea District*, p. 26.



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circumvallation can be clearly followed. On the flatter land sloping towards the Dead Sea the wall is set at intervals with towers. On the north, west and south it crosses deep ravines, and climbs to the summit of lofty cliffs standing almost on a level with the fortress. On the line of the wall, but a short distance behind it, are the two largest camps, B and F, each containing a superficial area of about five acres, which must have held Silva's legionaries. In addition to these there are six much smaller camps, which were probably garrisoned by auxiliary troops. The great mound rises from the saddle on the west, and near its base is the enclosure where the material for the attack was assembled.

To the examination of the site, Professor Schulten and his colleagues devoted four weeks. While he reproduces plans of the buildings which are still to be seen on the summit, his work has been chiefly devoted to the study and planning of the Roman military works, and, in particular, the camps both legionary and auxiliary, which he has examined with a completeness which has never been attempted before.

The camps, for they were essentially laid out for temporary occupation, differ widely from the forts with which we are familiar in Britain or in Germany. The element of permanency suggested by the central principia, the granaries, the bath houses, are wanting, and yet the remoteness of the site has secured for its drystone constructions a permanence which cannot be matched in the west. The plans present a picture of the layout of the camps of a force on active service. There are no inscriptions available to aid us in identifying the various elements which composed Silva's army. We know that the Xth Legion Fretensis took part in the siege, and the legionaries must have occupied the two larger camps B and F; the first of these being probably the headquarters while the circumvallation was in progress, the second when the construction of the mound was bringing the siege to a conclusion.

Professor Schulten has worked out from the existing remains, aided by the text of Hyginus and other sources, that the military force present and quartered in the eight camps numbered about 8000 men. In addition to the fighting strength he estimates that 2000 non-combatants would be required to bring water and stores to the army. The superficial area of the eight camps is the equivalent of about 13½ English acres. The area is astonishingly small when we consider that at Caerleon, York, or Chester, the legion occupied about 50 acres, while for an auxiliary cohort 1000 strong, such as the Tungrian garrison of Housesteads, 4½ acres were allowed. If Professor Schulten's estimates are correct, the men must, to use his own words, have been packed like sardines.

An interesting problem centres round the camp F, which from the presence of *canabae* gives evidence of more intensive occupation than any of the others; within the camp, and occupying one of its angles, there has been inserted a smaller and much more strongly fortified redoubt F.2. Von Domaszewski interpreted this as a place of deposit of the siege material. Mr Hawkes regards it as having been fortified to house the garrison left behind after the siege. Professor Schulten sees in it a construction dating from the Diocletian period, and cites as a parallel late Roman forts such as Alzei and others. The plan does not seem to support this view. The gates of the original camp F were each provided with a *clavicula*. When the more strongly fortified area was cut off from it a new gate was provided in the east wall, and this also was protected by a *clavicula*. The *clavicula* was an early feature which had disappeared in the forts of the late third and fourth century; its presence seems to prove that in point of time the construction of the redoubt cannot be much later than the camp.



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The presence of towers on the wall is not necessarily a late feature, but the towers of the forts of the fourth century, as at Alzei, differ from those of the Flavian and Antonine forts in their projecting outwards from the wall.

The series of plans of the forts and of the mound, as well as the general map showing the whole circumvallation, will be welcomed by all students of Roman military operations. A series of air-photographs is also reproduced, but compressed into smaller space they are less effective than those which were recently published by Mr Hawkes in *ANTIQUITY* (March 1931, pp. 60-97).

JAMES CURLE.

**ADAM'S ANCESTORS.** By L. S. B. LEAKEY. *Methuen, 1934. pp. XIX, 244, with 30 text-figures and 12 plates. 7s 6d.*

The atom is no longer solid, the Old Stone Age no longer simple, whilst Mousterian man himself begins to look lamentably synthetic. Dr Leakey, thinking over his adventures in East African prehistory, and correlating with these a wider survey of the Pleistocene field, gives full value to his readers, who should be numerous. He is not the only one who is engaged in complicating our simplicities, and even the most cautious student will bear him no grudge for exercising his well-earned right to have his say—particularly since he says it with an air of conviction that is often infectious. In expressing conclusions based on the results of recent work in East Africa, Egypt, Palestine, China, and various parts of Europe, he has shown a boldness which is not, or is not regrettably, dogmatic, and if at times the evidence is insufficient to convert, it is usually strong enough to support his theses, provisionally. In any case, archaeology is a fascinating jig-saw puzzle, with so many missing pieces that those which come to hand, mutilated and discoloured, must often be laid down in the wide open spaces. One guesses, much or a little, and certainties emerge in course of time.

Dr Leakey writes in clear and simple language, and though the 'general reader' will sometimes find himself beyond his depth—as he should expect—the book will be welcomed not only by many whose interest in the subject calls for tender treatment, but by those whose faith is tempered by experience. His illustrations range from adequate to excellent, the latter predominating. His discussions of discoveries and theories which have not yet got into general circulation in this country are of especial interest, even though at times one must wish that the proportions of the book had allowed him to offer us a wider choice of hypothesis or theory.

Only a few of the more important aspects of the book can be touched upon here. Amongst the most significant, as marking tendencies that have of late become conspicuous, are the emphasis laid upon the contemporary development of various Pleistocene cultures, often with interaction between them; the primary distinction drawn between 'flake-cultures' and 'core-cultures'; and the recognition of a number of successive stages of development in cultures (such as the Clactonian and the Levalloisean) which a few years ago had scarcely established their footing. By extending the Pleistocene downwards—as others have done—Dr Leakey brings all the lower ('colithic') cultures within this period, with the Kentish type as the earliest. The other three East Anglian 'cultures' he regards as contemporary with each other, two of them persisting into Günz glacial times. With the Günz-Mindel interglacial we get Chellean I and Cromerian, the former passing into Chellean II just before the Mindel glacial, and so to Acheulean in the Mindel-Riss interglacial. By the time of Würm I, the Acheulean has reached its stage VII, or Micoquian. The Cromerian gave rise to the Clactonian, which



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passed through stages I to III (High-Lodge culture) during the Mindel-Riss, and in turn gave rise (in France) to the Tayacian, about the time of Würm I; out of this developed the true Mousterian, which flourished between Würm I and II, but which is regarded as rather a restricted culture, by no means so important or so widely-spread as was formerly supposed, and is perhaps not represented in England. The Levalloisean, on the other hand, beginning with a 'proto' stage in the Mindel-Riss, reached its stage VII soon after Würm I, and the result of its influence on the late Acheulean was the Lower Aurignacian, perhaps in part in continuity with a basal Aurignacian of the Riss-Würm.

It will be seen that Dr Leakey is not afraid of shocking the old school—or even the new, in so far as this has not become immune—by dint of controversy.

Of 'Adam's Ancestors' themselves, and of their nearer relations, Dr Leakey has much to say that is important for our genealogy. Basing his decisions to a large extent upon his own East African discoveries (Kanam and Kanjera skull fragments) he assigns to *Homo sapiens* the Chellean and Acheulean cultures—as well as, of course, those from the Aurignacian onwards—whereas the earlier flake-cultures, Clactonian, Levalloisean, and Mousterian, he attributes to what we may call Neandertaloids, which, incidentally, he groups in the genus *Palaeoanthropus*. Our so-called Mousterian or Neandertal man he regards as an artificial or composite species, since the Mousterian culture has had assigned to it much that in reality belongs to the other two early flake-cultures, and since, therefore, not all the human remains that have been called Mousterian are entitled to the name, allied to Neandertal man though they surely are.

The book is so provocative of discussion that discussion is impracticable within the limits of a brief review. It is not all new, but it is new enough. One would like to believe it all, and so perhaps outstrip the author, but it must be sufficient to admire the picture he has put together, and to give some idea of its general character. The reader is referred to the book itself for Dr Leakey's expert treatment of flint fracture, of methods of working and using stone and bone, and for other conclusions concerning early human types, drawn from his own work in East Africa, as well as from recent discoveries in Palestine and China. Whatever our reservations—and he would be astonished if they were not many—we must congratulate him on the production of a 'popular' book which is learned and ingenious in its reconstruction of the prehistoric past, and fertile in suggestions for the prehistorian's future.

H. S. HARRISON.

**HABITAT, ECONOMY AND SOCIETY:** a geographical introduction to Ethnology.

By C. DARYLL FORDE. *Methuen*, 1934. pp. XIV, 499, with 109 illustrations and maps. 15s.

A generation of English geographers, intoxicated by the doctrines of Ratzel and Le Play, so simply and seductively presented to them by the genius of the late Professor Herbertson, proceeded to 'explain' man and his work in terms of environment. Historians, anthropologists and archaeologists who turned aside to rebuke these enthusiasts discovered unexpectedly a geography that compelled attention. Hence that growing awareness and appreciation of the geographical factor which informs and vivifies, for example, the earlier chapters of Professor Trevelyan's *History of England*, and gives its very texture to Sir Cyril Fox's *Personality of Britain*. University students of ethnology and geography are thus well disposed for that mutual introduction which Professor Ford's book is designed to effect. Doubtless it was in his mind that a strong dose of anthropology was the best cure for Geographical Determinism, but he was also aware

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that the meagre and perfunctory description of habitat in current works of anthropology was a symptom of a deficiency disease.

The plan of his book is simple. Peoples of a relatively low cultural level are roughly classified according to their fundamental economy as food gatherers, cultivators and pastoral nomads. Examples of each class are described as they occur in four of five strongly contrasting geographical regions. Thus the student is immediately disabused of the notion that there is a necessary genetic relation between type of region and mode of life. Food gatherers are found from equator to pole; cultivators make use of digging stick, hoe or plough; pastoralists herd a wide variety of animals, from camel to reindeer. Careful objective accounts of over a score of specific human groups, divorced from any attempt to theorize about them, bring out the diversities of ideas, of aptitudes, of social and cultural inheritance which intervene between man and his habitat. But an excellent series of specially prepared maps and line drawings (from photographs) keeps the student in mind that between man and his habitat a relationship, although not necessarily a causal one, must always exist. The amount of detail necessary to the descriptive sections (more than three-quarters of the whole) makes them rather heavy reading, but each chapter is self-contained, and can be studied separately.

In part IV are to be found the guarded conclusions which can be drawn from a review of the objective studies, followed by an account (which will be peculiarly welcome to geography students) of the most acceptable views on the origin of cultivated plants and the domestication of animals. In the concluding chapter, Professor Forde points out that 'broad general classifications of climatic or vegetational regions are quite inadequate for the analysis of cultural possibilities . . . Between any two broadly similar regions, the contrasts in detail in the precise conditions that are effective in the culture . . . outweigh the significance of any generalization concerning them all'. It is here that recent advances in geography will be of service to the anthropologist, for the broad categories of major regions being fixed, attention is now being focused on the 'precise conditions' of the individual area, and a technique of landscape description has been devised which should form part of the equipment of every traveller and field-worker. From this point of view it is a pity that the author did not include Professor S. Passarge's *Beschreibende Landschaftskunde* in the otherwise admirable bibliography which is appended for the further guidance of students.

A word of praise is due to the pleasing line-drawings which are always truly illustrative. The artist, Mr Anthony Brown, was for some time resident in West Africa, and it is clear that his contact with native peoples and cultures there has enabled him to grasp exactly what the ethnologist requires. This is a book which lately has been consciously needed; and a serious gap in the student's library has now been admirably filled.

E. G. R. TAYLOR.







THE SO-CALLED SHEIKH-EL-BELAD  
Cairo Museum  
(See *ANTIQUITY*, March 1935, p. 14)



# Antiquity

## A Quarterly Review of Archaeology

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### Editorial Notes

OUR Editorial Notes are usually addressed to all who may read them; but for once a more limited audience is visualized, though we hope that all will read and be interested. We are writing mainly for the contributor this time. There are several ways in which those who write articles for publication in *ANTIQUITY*, or for any other illustrated journal, can assist the Editors. It is, for instance, the business of every writer, and particularly of those who do research work, to get to know the processes involved in publication; how maps should be drawn for reduction, and how photographic prints should be made, and so forth.



It is necessary to acquire this knowledge because the work of research is not complete until it has been published. Without it no archaeologist, therefore, is properly equipped. This is not the place to give a course of instruction in how to submit articles (or books) for publication, but the subject might with advantage be added to the syllabus of courses in archaeology. It is, for instance, no part of the editorial duty either to find, or to make illustrations, though, because



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he wants the article, the Editor often does both. Maps, too, should be drawn with due regard to the size of a page in the journal for which they are intended, and for the amount of the reduction they will have to stand. Articles should begin at the beginning of the subject treated and not in the middle, and should end at the end. They should also be written in plain, straightforward English, with as little technical jargon as possible.



We hasten to add that these defects appear only in the articles rejected, and further that most of the work thrust, as we think, unfairly upon Editors is the result of lack of knowledge and not from mere laziness (though that may sometimes contribute!). We are often complimented upon our illustrations, but the secret of them is quite open—to get good negatives and make *good, enlarged* prints from them. This could be done, and should be done, by the contributor in the first instance, when he is submitting the article; actually it very rarely is. In most cases it is necessary to write for the negative, sometimes (as we had recently) to the Antipodes, only to discover that the negative is lost and nothing but a poor little contact-print is available.



Up to now we have usually done the work and said nothing, but in future we may become more hard-hearted, and return articles that would entail the extra work we have referred to above—we think it would be quite reasonable to do so and thus conform to the usual practice.



The excavating season will soon begin in grim earnest. Many important sites are to be tackled, some for the first time. Maiden Castle will no doubt be the centre of attraction, as it deserves to be. An article by Dr R. E. M. Wheeler has discussed in *The Times* of 1 March last the results obtained from the first season's work. Of the smaller digs we shall watch the excavation of the Norwich Woodhenge with great interest. Ireland is well to the fore as last year, and Dr



## EDITORIAL NOTES

Hencken's expedition will no doubt continue their pioneer work. Dr Hencken has chosen for himself a big task, but he has big ideas and is going to make archaeological history on a large scale.



In a letter to *The Times* (13 February) Mr Harold Peake, F.S.A., asks the Conservator of the Museum of the Royal College of Surgeons for the evidence upon which he attributes the Ovingdean skull to the Bronze Age (Mr Peake says 'the late Bronze Age'). It is true that, as Mr Peake points out, the inhabitants of England at that date usually cremated the bodies of their dead; so that naturally we have no evidence of their head-form. But even if we had it would be impossible for an anatomist to say 'this is a Bronze Age skull'. (See also MAN, 1935, nos. 55 and 56).



It is impossible for an anatomist, acting as such, to assign a skull to any given period, that is to say, to date it; to do that is the business of the archaeologist, and he does so from associated grave-goods, when present, or from the type of grave it was found in. *Ex cathedra* pronouncements of this kind are unscientific and misleading. The bases of physical anthropology, and the racial inferences built upon them, need re-examination. The future lies with such devices as the coefficient of racial similarity, so far as physical anthropology has a future. Personally we think too much attention has been focussed on race and racial differences. The results are apt to stimulate just those passions that lead logically to war and should rather therefore be damped down.



How revolutionary the new method can be is shown by some of the results already obtained. Like most of his generation the present writer was brought up to believe that the neolithic inhabitants of Britain were 'small dark Mediterraneans'. He ventured to question the correctness of this theory some twenty years ago, and was supported by one who is now an eminent anthropologist. It appears that he was

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right, and that the neolithic long-barrow skeletons have the closest affinity with the inhabitants of the North German plain. In other words, our neolithic ancestors belonged to the Nordic long-heads, not to the Mediterranean long-heads. In modern terms this means that they were more akin to Norwegians than to Sicilians. (For references see ANTIQUITY, 1934, VIII, 106).



The promised article on 'Arthur's Battles' has again been unavoidably held over but we hope to publish it definitely in our September number. It will be followed by one on Western Seaways, dealing with intercourse between the Celtic west and the Continent during pre-historic times and the Dark Ages.





PLATE I



SUMERIAN REIN-RING AND 'MASCOT' FROM QUEEN SHUB-AD'S CHARIOT POLE (See p. 134)  
*By permission of the British Museum.*



# The Evolution of the Domestic Horse\*

by DR MAX HILZHEIMER

THE term 'horse', usually taken by the lay mind to apply only to the domesticated animal, has a much wider connotation for the zoologist, who extends it also to include the zebra, the grey African ass, and the Asiatic dziggetai which varies in colour from yellow to reddish brown. The latter derives its name (*hemionus*) from its appearance, which is something between that of the ass proper and of the horse proper, although actually it is not closely related to either group, and in fact is better regarded as quite independent.

Until very recently, only the ass and the horse proper were known to have been used for domestic purposes, but the latest excavations in Mesopotamia have led to the astonishing discovery that the early Sumerians, about 3000 B.C., tamed the local breed of Asiatic dziggetai, *i.e.*, the onager of hither Asia. However, this breed seems to have been confined to that period and to have become extinct with the destruction of the old Sumerian kingdom, for we find no trace of it among the later Sumerians. Woolley's discovery of pictures of animals resembling horses drawing war-chariots<sup>1</sup>, in the royal graves of the early Sumerian kings at Ur, naturally took the scientific world by surprise as being something entirely novel. Before this, there was no evidence that the Mesopotamian dziggetai was ever domesticated, so that the find caused much discussion as to the precise nature of these animals. A few scholars took them to be horses, but the majority, including the writer, preferred to regard them as mules, while Woolley himself at once named the dziggetai. His opinion, however, was thought improbable by zoologists, not only in view of the small size of the onager (just over 10 hands), but also because the taming of these creatures was held to be quite impracticable, all modern attempts to tame them being reported as failures. But I have lately received from an American source certain collections of bones, not indeed from Ur, but from Tell Asmar, among which are very many remains of the onager but none of other animals resembling the horse: this, therefore, confirms Woolley's theory that the onager was used as a draught-animal by the early Sumerians. The

\* Translated by Roland G. Austin, Glasgow University.

<sup>1</sup> See Woolley, *The Royal Cemetery*, pp. 271 ff, and plate 92.



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zoologists' mistake can be easily explained; the mule, produced by crossing the ass proper with the horse, so closely resembles the dziggetai (itself midway between the two parent types) that the two can hardly be distinguished from each other in primitive art, where exact details of breed are seldom stressed. However, in his main work on Ur,<sup>2</sup> Woolley has recently given us a fine picture of such an animal from an early Sumerian rein-ring; this clearly shows all the points of the dziggetai—short ears, erect mane, and stalk-like tufted tail (*gestielter Quastenschwanz*) like that of an ass (PLATE I). We must therefore regard the domestication of the onager as peculiar to the early Sumerians, not recurring elsewhere. This is suggested also by the unique method of bridling, with no bit, but instead a strap curving round the jaw. It is difficult to decide whether this innovation was due to external influences.

About this time the ass had already been domesticated in Egypt and the horse in Europe. We do not know when this took place in Egypt, but the ass appears along with others as a definite domestic type on a slate palette of an early period; it is uncertain whether, still earlier, prehistoric, examples represent the domestic or the wild ass. At a later period, asses occur with remarkable frequency in Egyptian art; they were used chiefly as beasts of burden, and perhaps also for treading corn, while occasionally they appear bearing a frame (*Gestell*) for carrying a woman. The ancient Egyptians did not use the ass for riding or as a draught-animal; in fact, in early times we know of only two peoples who were accustomed to ride on asses—the Jewish patriarchs, if indeed we do not misunderstand the Biblical tradition, and the Nubians. In the case of the latter, this practice may be inferred from some 6th century bas-reliefs, now in the Berlin Museum, but it must certainly have been suggested by the analogous use of the horse, which had already been known in Egypt for a thousand years. We do not know when the ass began to be bred outside its native Egypt, but this seems to have occurred very early—at the beginning of the third millennium. However widely distributed these animals were, the peoples of the ancient East (with the two exceptions already mentioned) never used them for any other purpose than as beasts of burden, and they were not bridled but always driven. This distinction between the ass and the horse cannot be too strongly emphasized, for it is a clear proof that both animals were employed for domestic purposes independently and with differing ends in view. We do not know whether in those

<sup>2</sup> *Loc. cit.* p. 78, and plate 166.



## THE EVOLUTION OF THE DOMESTIC HORSE

days there were already varieties of breed, such as the fine-bred white asses of Muscat, or the enormous beasts of Poitou, Catalonia, etc., or the pigmy Cingalese breed. Early Egyptian art shows only one type, and probably the development of breeds did not set in until a later period. The domestic ass is pre-eminently at home in warmer countries, as one might expect from its African origin; it is comparatively rare in colder lands, and even the climate of central Europe does not seem to suit it.

The domestic horse presents quite another problem. Although this animal avoids the tropics proper, it is otherwise widely distributed throughout the whole world. But the old world is its real home; it was there that the wild horse flourished during the glacial epoch, even passing thence into North America. We have many remains of bones from this period, as well as many representations, apparently very true to life, from palaeolithic cultural sites in western Europe. It is possible that one may distinguish two and perhaps three different types. But it is untrue to assert, as was once done, that these horses of the glacial period were ever domesticated. Domestication only begins in the present geological epoch, and the existence of the domestic horse in Europe cannot be definitely proved until towards the end of the later Stone Age, in many instances not until the Bronze Age. As for Asia, there has hitherto been no investigation into the prehistory of the home of the wild horse there. It appears that, at the period when domestication began in Europe and Asia, wild horses only existed north of the great mountain chains, with the exception of the extreme West, where they seem to have crossed the Pyrenees and to have reached Spain. The habitat of the wild horse appears to have stretched eastwards from the sea-coast right across mid-Europe and central Asia as far as Mongolia, where the last living wild horses are found today. Northwards, it reached as far as central Sweden and Siberia. It is in this enormous region, therefore, that we must look for the home of the domestic horse.

Here we have to deal, as it seems, with two types, the so-called Przewalski's horse<sup>3</sup> and the Tarpan<sup>4</sup> (PLATE II). Their respective habitats are divided by the 40th degree of longitude.<sup>5</sup> The Tarpan lived to the west of this line, and until last century was still to be found in the Steppes of south Russia. It is described as a small mouse-grey

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<sup>3</sup> *Equus equiferus* Pallas.

<sup>4</sup> *Equus gmelini* Antonius.

<sup>5</sup> Roughly the longitude of Archangel and Rostov-on-Don.



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horse with black eel-mark, black or slightly striped legs, heavy head with a concave profile, soft erect mane, and a tail quite covered with hair. A somewhat divergent type, the so-called Forest Tarpan, existed in the forests of western Europe, as Vetulani among others has lately shown. It is not known how far the Tarpan spread westwards, or whether other breeds originated the types described to us in a tradition which reaches to the Middle Ages. Professor J. C. Ewart assumes another breed for western Europe, which he terms *equus Celticus*, basing his theory on the definitely divergent types of pony in north Britain and in Norway. In any case these wild horses of western Europe appear to have been more markedly striped, for occasionally even today some of the Norwegian domestic horses, evidently their descendants, can be found striped exactly like a zebra.

East of the 40th degree, over the whole tract of the Steppes of Europe and central Asia, lived Przewalski's horse (PLATE III). This breed is for the most part extinct in its original habitat, and survives only in the extreme east, in Mongolia, though rarely found. It is some 13 hands at the withers; it is of a brown colour, eel-marked, bearing traces of a cross on the shoulder, with stiff erect mane, short hair on the upper part of the dock, heavy long-muzzled head with straight to convex profile. It is not certain whether wild horses exist today still further east of those regions, but mysterious tales recur of white ones being seen in the neighbourhood of the Jana. But such animals certainly have no bearing upon our inquiry into the origin of the domestic horse, which could only have been evolved from the Siberian breeds already mentioned and now probably extinct—if indeed these differed from Przewalski's horse, and after all we only possess fragmentary remains of them. What then is to be the starting-point in our inquiry? Must we consider several centres of domestication? How did the domestic horse spread from them? To answer these problems we must attempt a brief sketch of the classification of the domestic horse.

According to an early method of classification, the domestic horse is divided into oriental and occidental breeds. These two contrasting types may be roughly compared by taking the modern Arab (PLATE III) and a heavy horse such as the Clydesdale (PLATE IV) or the Rhenish-Belgian. But certain European breeds also belong to the oriental class, among others the Konink (PLATE V), a breed native to Poland which has lately been the subject of investigation by Polish scholars. It is a very ancient breed, and was indigenous to Europe in prehistoric times. This is an excellent proof that the so-called



PLATE II



THE TARPAN (See p. 134)  
from Jardine's 'Naturalist's Library.'

PLATE III



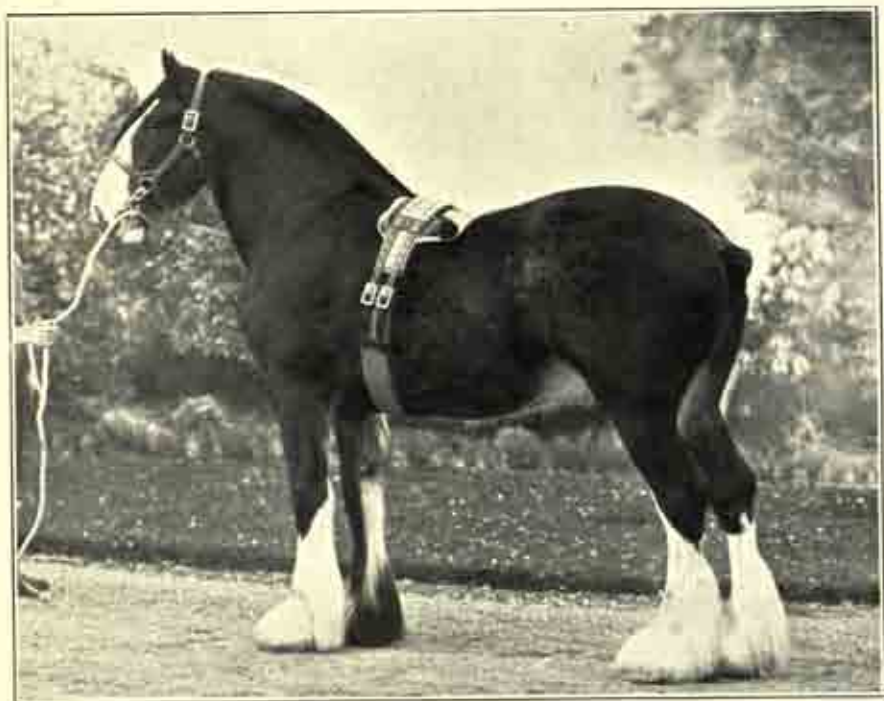
(1) PRZEWALSKI'S HORSE (See p. 136)



(2) ARAB HORSE—NUREDDIN II (See p. 136)  
bred (1911) by the late Mr Wilfrid Blunt  
*By courtesy of the Arab Horse Society*

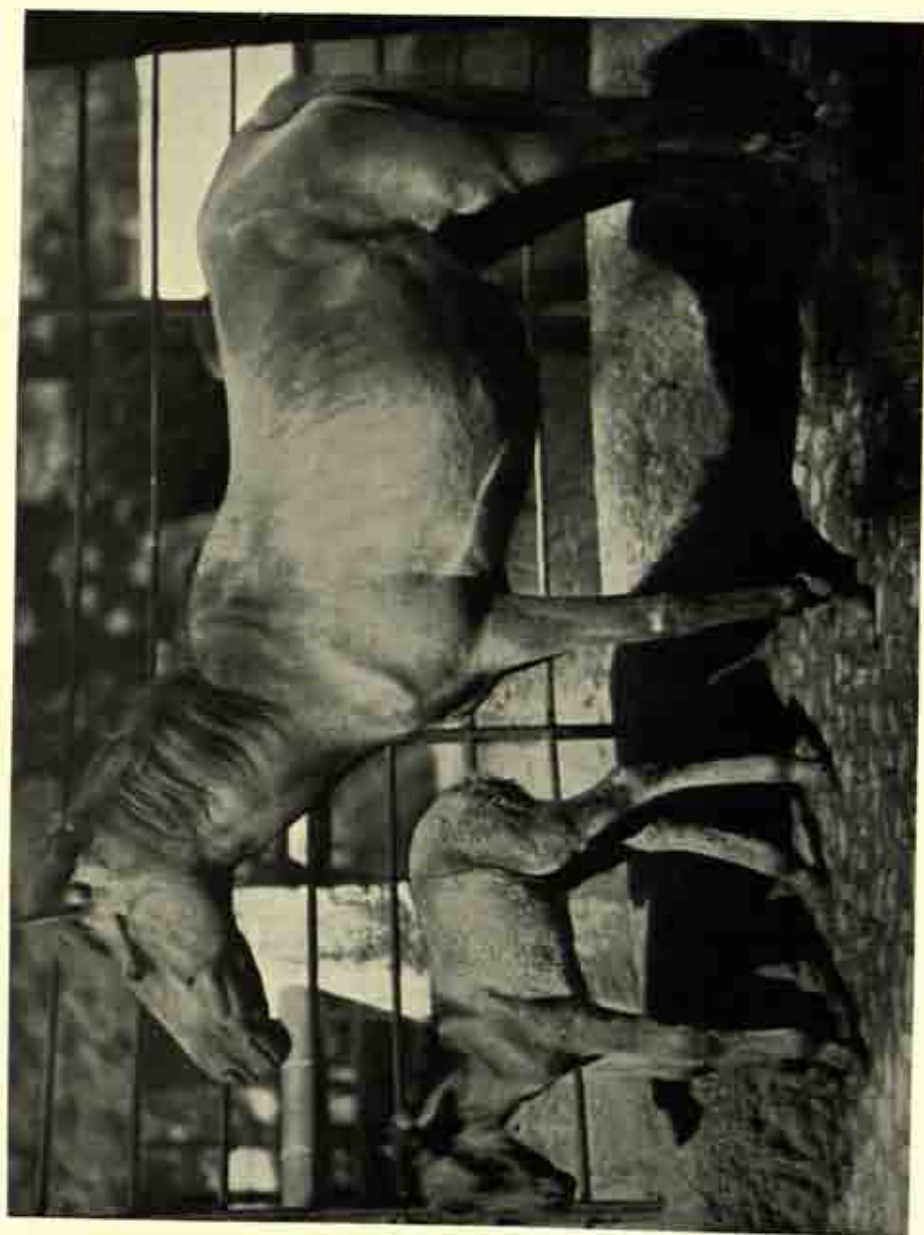


PLATE IV



THE CLYDESDALE (See p. 136)  
*By courtesy of the Clydesdale Horse Society*

PLATE V



POLISH KONIK (See p. 136)



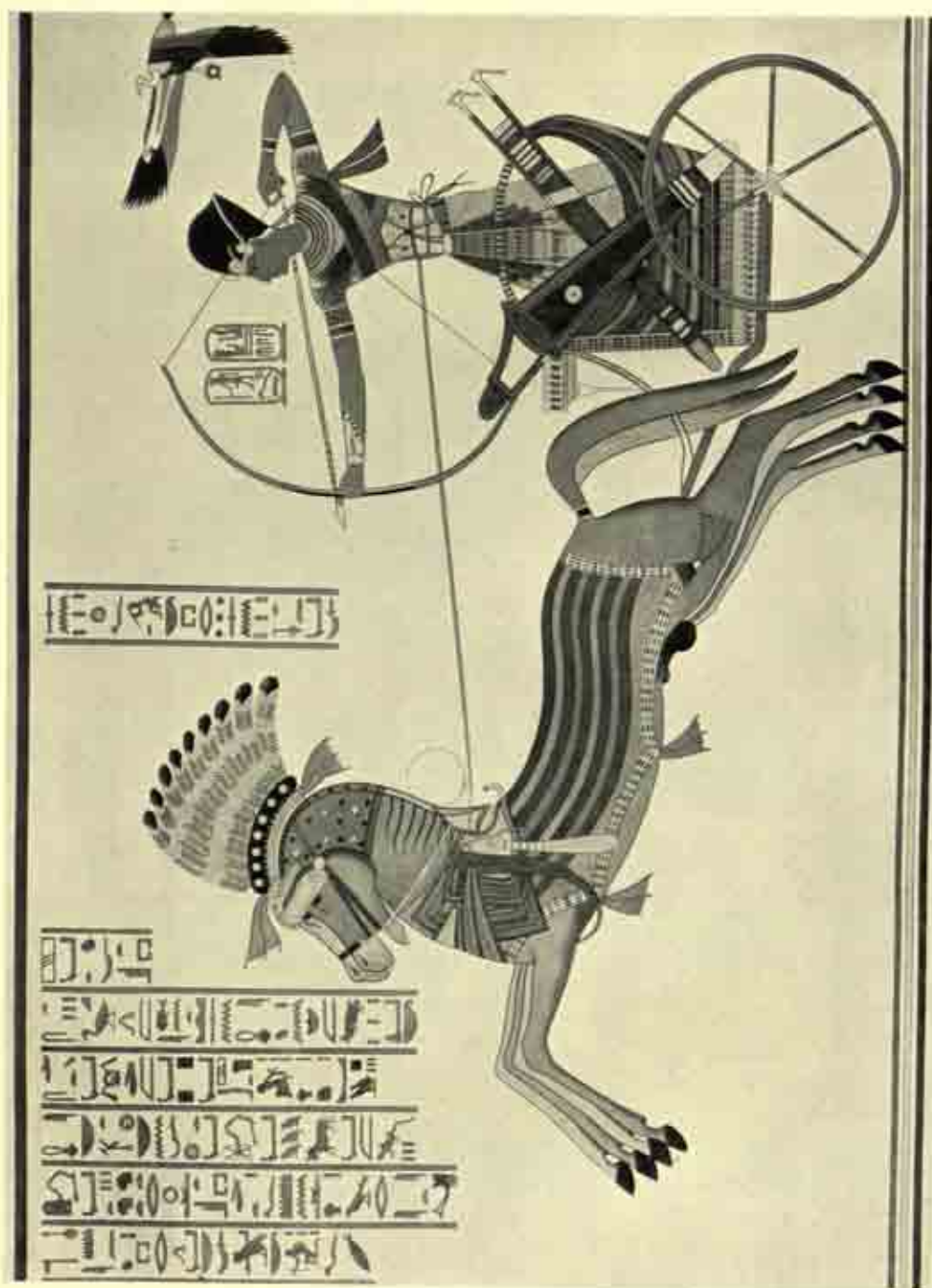
PLATE VI



EARLY ASSYRIAN HORSE- WAR-HORSE OF ASSURBANIPAL. (See p. 132)

*National Museum, Berlin.*

PLATE VII



EGYPTIAN HORSE AT ADU SIMHEL (See p. 137)  
 reign of Senusert III (1882-1849 B.C., Brinsford)  
 from A. E. Champollion, 'Monuments de l'Égypte et de la Nubie'



PLATE VIII



HITTITE HORSE. (See p. 238).  
from H. Meyer, 'Rösch und Kultur der Chetier'.

PLATE IX



REPRESENTATIONS OF WILD HORSES AND SCYTHIAN 'TAMERS' ON THE SILVER VASE OF CERTOMLYS  
(See p. 138)

from Ebert, "Reallexikon", Fig. 1340 and b



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oriental group did not originate in the East ; and certainly not in Arabia, since horses were not found there until about the time of the birth of Christ, and the fame of the Arab breeds coincides with the spread of Islam. The original home of the oriental horse is to be sought rather in Europe, as most investigators now assume, particularly in the Steppes to the north of the Black Sea, where the now extinct Tarpan had the same skull-formation, *i.e.*, relatively broad brow, great round eye-sockets, slightly concave profile, and a face that was short in proportion to the skull. From this region these horses spread eastwards and southwards, and gave rise there to a number of fine breeds, which owe their excellence partly to the extraordinary skill in breeding possessed by the oriental races and partly to the extremely favourable climate. Although these highly-bred oriental types are quite unlike the low breeds still native to eastern Europe today, such as the small Lithuanian pony or the Polish Konink and other kindred species, yet they are derived ultimately from the same stock, and show the difference produced by a favourable climate and skill in breeding on the one hand and by a bad climate and indifference on the other. The earliest of these breeds may be seen on Assyrian monuments dating to the first half of the first millennium, represented in a most life-like manner. Actually, horses were introduced into Assyria considerably before this ; our earliest evidence for them, though scanty, goes back to the third millennium ; but they began to be more numerous from the middle of the second millennium onwards, and to assume importance as animals which could be used in war either by the Assyrian cavalry or to draw the light war-chariots. Horses appear to have reached Mesopotamia from the north, probably together with the Aryan tribes, which spread as far as Egypt in their wanderings (*i.e.* the Hyksos) and imported the horse thither about 1800 B.C.—it was quite unknown in the early and middle kingdoms. The early Assyrian horse (PLATE VI) was a big, sturdily-built, noble animal, comparable perhaps to the modern Hanoverian. The early Egyptian horse (PLATE VII) was a smaller and lighter breed, probably one of the chief ancestors of the modern Arab. It seems to have reached Egypt not from Mesopotamia, but rather from races living further to the west. At any rate it resembles more the horses of the Hittites, which, though drawn in a crude and clumsy style, yet afford evidence of a small light type, shown invariably with a characteristically high carriage of the tail (PLATE VIII). The horse of the early Egyptians also presents this very same characteristic (*Abspreizung des Schwanzes*), as do many Arabs of today, a fact which establishes,



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with much probability, the relationship of all these breeds. In quite early times there was another oriental type, which betrays unmistakable features of the occidental horse, to which we shall return later. The other breeds of classical antiquity very likely go back to the Tarpan. Such a type is found in Greek works of art, especially in the life-like representations of the Parthenon frieze; these are small stocky ponies with a somewhat thick-set neck, but otherwise highly-bred. The early Roman cavalry-horse also appears to belong to this group, as it is shown for example in the only extant equestrian statue of Marcus Aurelius, or on the pillar of Trajan, and in countless other representations. Greco-Scythian art proves that fine highly-bred beasts were to be found in the home of the Tarpan. If for example we look at the 'horse-taming' Scythians on the so-called silver vase of Certomlyk (PLATE IX), it is not unlikely that here we have depicted the actual capture of wild horses, indeed the wild Tarpan itself. In fact it is from this splendid, natural picture that we gain probably our best impression of the extinct wild horse of south Russia. It remains to hope that the numerous skeletons of horses found in the Kurgans of south Russia will some day be examined and correlated by an expert.

Let us now turn to the occidental group. The name is derived from the fact that their origin was assumed to be in the west. The type is said to go directly back to a heavy wild horse, whose bones are found in the glacial deposits of central Europe. But it is remarkable that such a heavy horse has never occurred in prehistoric sites. Neither are there any representations of such a horse extant from prehistoric Europe; much less are we told by classical writers of any breeds of horses which are obviously of the heavy draught-horse type (*Kaltblütig*). One of the earliest representations of a horse showing the points of the occidental group occurs in the tombstone of Albanus, an Ubian, at Châlon-sur-Saône, dating to the 2nd century A.D. This has all the characteristics of the heavy draught-horse: here are the arched croup (with the tail so to speak inserted into it), the relatively small head as compared with the size of the animal, the flat hoofs with the fetlocks above, the curly hair of the mane (only found in such horses), the poise of the neck, the mould of the head, the heavy figure with powerful chest, the peculiar action of the forelegs: in all these points it resembles the heavy cavalry-horse of the later Middle Ages. The heavy draught-horse is first clearly to be found in Europe in the region round about the North Sea coasts (in the wide sense of that term). About two hundred years later, a characteristic picture of it can be seen on a helmet-fitting



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from the tombs at Vendel. This sudden appearance of the draught-horse in north-central Europe has led me to think that we are here concerned not with a type which traces its descent directly from a wild horse of like appearance, but with one which was produced as an exotic by skilful breeding from the common type, on specially favourable soil. My view is supported by the dual appearance of this type in places geographically separate—once in the northeastern Alpine regions (the so-called Noric horse), and then again, but much more widely spread, in the East, in ancient Persepolis. Here we find, from about the fifth or sixth century B.C., a small pony-like horse, exhibiting clearly all the marks of the heavy draught-horse type as I have described them above; the breed lasted till the period of the Sassanids. Although for some years I have laid increasing emphasis on the importance of this peculiar breed, investigators into the history of domestic animals have paid practically no attention to it; presumably because this characteristically draught-horse type, appearing suddenly here, did not fit in with the theory by which the occidental horse was derived from a heavy wild horse native to Europe. Actually such a breed seems to have existed there once, but only during the glacial period, and apparently did not outlive that epoch anywhere. Thus it became extinct several thousand years before the horse was domesticated, so that this break in continuity alone suffices to rule out the theory that the heavy draught-horse type is descended from the wild horses of glacial times. If in spite of this there is a certain similarity between them, it can be explained in either case by my theory of an exotic breed produced in specially favourable circumstances. The types would naturally resemble each other, and in fact are an example of what zoologists term convergence.

In this way it appears that at least the European horse, including the so-called oriental horse of western Asia and the breed of northern Africa, is to be derived from the European Tarpan in its various subdivisions. The Barb is perhaps an exception; our information about its history is at present very inadequate.

At the moment there is no conclusive explanation available as to whether we must assume the existence of a separate centre of domestication and a different primitive type in the case of the horse of central and eastern Asia.

## Waggon and their Ancestors

by R. H. LANE

THE main purpose of this article is to put on record material from two collections of photographs, made by Mr O. G. S. Crawford and by the writer. As the 'exhibits' were varied and numerous, it seemed desirable to make a selection that would link up with what has already been published on the subject in *ANTIQUITY*, producing in the process some shadow of an evolution. For any opinions that are advanced, however, the writer, and not Mr Crawford, must be held entirely responsible.

Few references will be made to places outside Europe (or rather certain parts of it) or to times earlier than the 11th century. But as Sir Cyril Fox points out, the most primitive types are still to be found in the same districts as the highly developed ones, and there can be few types of vehicle for transporting goods by land that have not been in use within these limits.

The development of such transport (for with passenger vehicles this article is not concerned) is governed by four main factors—the load to be carried; the road or track by which it travels; the skill of the maker; and the capital available in the industry which it serves. In England, at any rate, legislators have often intervened, and their actions have sometimes modified a type in a way which has survived the legislation.

The vehicles that will result when these four factors are at a minimum were described by Sir Cyril Fox in part of his article, 'Sleds, Carts, and Waggon',<sup>1</sup> and I hope that he will accept my frequent reference to this as a sign of my appreciation. He shows us two types of wheel-less transport—the sled, developing into the wheel-car, and the 'v', made by tying two poles together at one end, supplying the possible ancestor of the truckle-cart.

There is, rather remarkably, evidence that vehicles of this most primitive stage survived very late, even in London. A cartoon designed

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<sup>1</sup> *ANTIQUITY*, June 1931.



## WAGGONS AND THEIR ANCESTORS

to ridicule the Excise Bill of 1733 shows what is virtually a sled, provided with very small solid wheels, which just raise it clear of the ground. The load is a barrel, and it is drawn by a lion and a unicorn. The cartoonist was presumably drawing something which was familiar in his day, though doubtless with a less remarkable team! In the 19th century an engraving from a volume<sup>2</sup> published in 1829 shows a sled, again loaded with a barrel, being drawn by a horse past the King's Weigh-House. The runners are curved, probably to reduce friction.

PLATE I, a photograph taken by Mr Crawford in Ireland, shows a vehicle which is definitely a cart, and not a glorified sled. It no longer suggests, as do some of the earlier type, that the maker kept his 'machine' as sled-like and as low as he could, just in case these new-fangled wheels 'let him down'. So it represents a second evolutionary stage. Much like this must have been the carts which Professor Thorold Rogers<sup>3</sup> says were common in England in the 13th and 14th centuries. The price of such a cart was, he says, under 3 shillings, complete with wheels, in the late 13th century, a time when this sum was less than half the value of a quarter of wheat. The wheels are described as plain or naked, which he interprets as solid and not bound with iron. Even so, the wheels cost as much as the rest of the cart, which must therefore have been, like the Irish cart, of very simple construction. He suggests that the plain wheels were mere sections of tree-trunk, but this I venture to doubt.

In any case, PLATE I is far from representing the crudest form of its type, for inspection shows an iron axle, iron tyres, nuts and bolts, as well as segmented wheels. It looks like (and doubtless is) a modern wheelwright's version of a traditional type, whose origins go back 'time out of mind', and the waggon-hub in the background shows that it by no means embodies the whole skill of its day. In this too, the scene has a medieval flavour, for in the 13th century solid and spoked wheels existed side by side.

The photograph shows the defects of this kind of wheel. It is easily damaged, and the bearing surface is small. Here it has been increased by an iron plate; in plate XVI illustrating Sir Cyril Fox's article it has apparently been increased by wooden blocks. Solid wheels will carry bigger carts than this, and they can be improved in several ways, such as cross-bars to strengthen them, and holes to

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<sup>2</sup> *London and its environs in the Nineteenth Century*, plate 83.

<sup>3</sup> *History of Agriculture and Prices*.



## ANTIQUITY

lighten them.<sup>4</sup> But if loads go on increasing, and capital too, the necessary skill will develop, and spoked wheels will come into being, though the solid may continue to be used, as they have here.

PLATE II shows this fresh stage in evolution. The cart has spoked wheels, on wooden axles, and its body, though still very crude, has some provision for a piled-up load. Its main interest is the striking way in which it reminds us of the 'v'. Divested of its wheels and rudimentary sides, it is just two timbers fastened together at one end, and kept apart at the other. Probably the natural strength of the triangle so formed gives it a high survival value. Its narrow fore-end moreover, is very suitable for ox-traction, but less so for horses. The latter can, of course, be harnessed with a pole and traces, as they are still in Yorkshire waggons, but shafts seem to be normal, and they produce a rectangular cart.

This photograph was taken by Mr Crawford in Tiflis recently, but similar carts were once in use in England. Two are shown in the 11th century Cotton MS. One (PLATE III), used for carting logs, is very similar to PLATE II. Its sides are of wooden uprights, higher than the modern example, with a cross-bar at the top. To carry the ox-yoke it has at the fore-end a fitting which looks like a pair of sugar-tongs with a pin through the middle, exactly like the device shown in PLATE IV, from Sukhum. The 11th century artist's perspective leaves something to be desired, but he can hardly be depicting anything but a triangular cart, like that of PLATE II. The second cart of the Cotton MS. is exactly similar, except that its sides are of wattle, a device which is as old as Homer, and also survives in another of Mr Crawford's photographs, from Transylvania.

In both these Cotton MS. carts, the wheels are built up with eight spokes and eight felloes. Such must have been the spoked wheels mentioned by Professor Rogers. He describes them as bound with iron in the form of separate pieces or 'strakes' first appearing in his lists in 1280 when they are a standard fitting, doubtless of long standing, but expensive, costing eight or nine shillings. It is not easy to make out anything of the kind in the Cotton MS. pictures, and there are curious dots which suggest a possible reason for their absence. There is much that might be said on the development of the ironwork on cart-wheels, but it would occupy too much space here. But it is well to notice that artists are not always trustworthy in minor details, outside

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<sup>4</sup> See ANTIQUITY, March 1928, 18 ff, and September 1929, p. 340.



## WAGGONS AND THEIR ANCESTORS

matters of which they themselves have knowledge. I have seen a modern highly-skilled draughtsman go wrong over the details of a waggon, of which he was making a separate drawing, and to many landscape artists a cart is just a detail in the middle distance.

The 14th century Louterell Psalter also shows a cart used for harvesting. The wheels, as well as can be judged from the human figures, are larger than in the Cotton MS. They are studded with large-headed nails, and have small (? iron) plates over the felloe joints. These are probably the 'gropes' of medieval accounts. Spokes and felloes are six in number. The cart is drawn by three horses in line, has shafts, and is parallel-sided, not 'v' shaped. Its chief interest lies in the fact that it has developed a projecting gallery in front, to increase the loading area. This survives in many present-day English carts, and is often so large as to hide most of the horse in the shafts. It is a permanent part of the cart, and not detachable.

As these wheels have fewer spokes than those of the Cotton MS., it is rather a surprise to find in Chaucer the statement, 'Twelf spokes hath a cartwheel comunly', for he is roughly contemporary with the Louterell Psalter. It suggests that the modern system of having two spokes to each felloe was then coming into use. Modern cart- and waggon-wheels nearly always have twelve or ten spokes, and the latter number is found in a cart represented on a 16th-century title-page,<sup>6</sup> with, again, a gallery in front; we have now reached a vehicle differing very little from a common type of 19th-century cart. It is designed to carry a big load, and shows skilful planning and workmanship. Capital had increased as the Middle Ages receded, but the development of roads lagged far behind, and it was the bad road-surfaces which were, I believe, responsible for the use of very large wheels.

Whether they are a development of the cart, as seems inherently probable, or originated independently, as some have suggested, waggons are certainly of great antiquity. Pliny attributes their invention to the Phrygians, and Hippocrates, a Greek writer of the 5th-4th century B.C., speaks of the Scythians as living in waggons, of which the smallest have four wheels, and the largest six. Scythia was rather a vague region, for the Scythians were nomadic, but it seems to have reached as far west as the Carpathians. PLATE V shows a present-day waggon from Bucharest, and Mr Crawford points out that in Slav countries today the four-wheeled vehicle is distinctly the normal,

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<sup>6</sup> Harman's *Caveat*, 1567.



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rather than the two-wheeled. It seems then, that the four-wheeled tradition in southeast Europe may be of very ancient origin.

Nearer home, the 11th century Bayeux Tapestry shows a very elementary four-wheeled vehicle. It is rather diagrammatic, but appears to be flat, without sides. It clearly has wheels with eight spokes, and is hauled by men, an interesting point in view of Sir Themistocles Zammit's opinion<sup>\*</sup> that the Maltese prehistoric carts were pulled by man-power.

In England, however, after the Norman Conquest, waggons were rare, according to Professor Rogers, though they are mentioned once or twice in his lists of prices. Their absence is understandable, for with medieval conditions of farming and trade, loads and capital were seldom great. Their adoption in England, at any rate, is therefore a later development.

There is evidence to show that both the waggon and its name were introduced into England from the Low Countries in the latter half of the 16th century. Stow tells us that 'in the yeere 1564, Guylliam Boonen, a dutchman, brought the use of coaches into England . . . and about that time began long wagons to come in use, such as now come to London'. (Boonen was, apparently, Queen Elizabeth's coachman.) This statement is supported by a proclamation of 1618 ascribing the decay of highways and bridges to the common carriers, who now use four-wheeled waggons drawn by 8, 9, or 10 horses, and carrying 60 cwt. at a time, where heretofore they used two-wheeled carts, carrying 20 cwt. In 1622 waggons were forbidden altogether, but their use was allowed with restrictions in 1661, and one appears, drawn by the statutory maximum of five horses, in Loggan's *Oxonia Illustrata*, 1674.

So far, these are, no doubt, mainly stage-waggons, carrying merchandise to cope with the rapidly increasing commerce of the time, but this 16th century also saw the rise of the independent farmer, who, freed from the limitations of 'open field' or 'strip' farming, supplied the growing towns with food for his own profit. Such men would have the progressive spirit which marks the epoch, and it is not surprising that we find some evidence of the existence of farm waggons at any rate in the 17th century. Two appear<sup>\*</sup> in a picture of harvesting

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<sup>\*</sup> ANTIQUITY, March 1928.

<sup>†</sup> *Oxford English Dictionary*: article 'WAGGON'.

<sup>‡</sup> Loggan, *Cantabrigia Illustrata*.

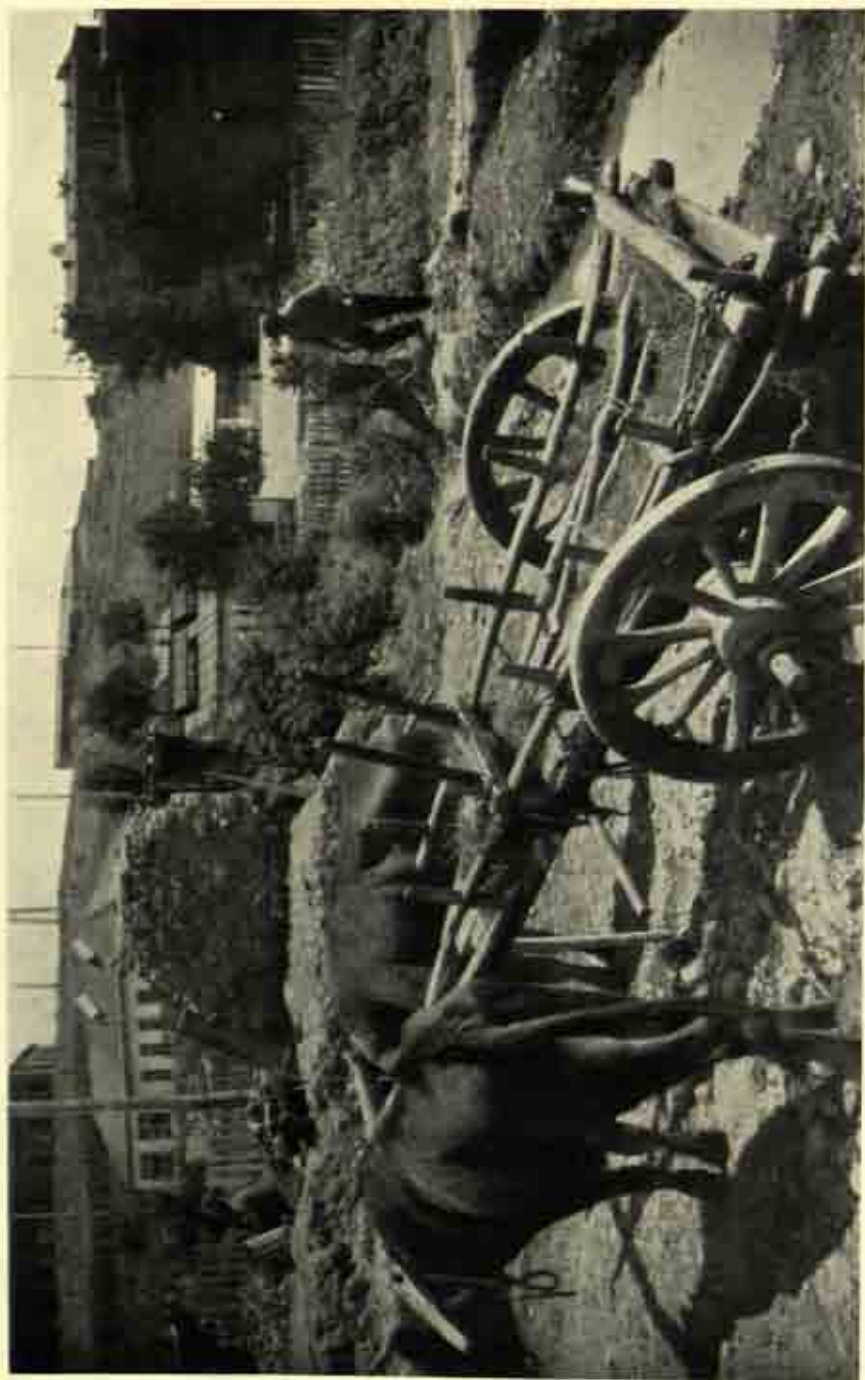


PLATE I



IRISH CART, TOWNLAND OF DRUMBONE, CO. MONAGHAN  
Ph. O. G. S. Crawford

PLATE II



OX-CART, TIFLES  
Ph. O. G. S. Crawford, 1932

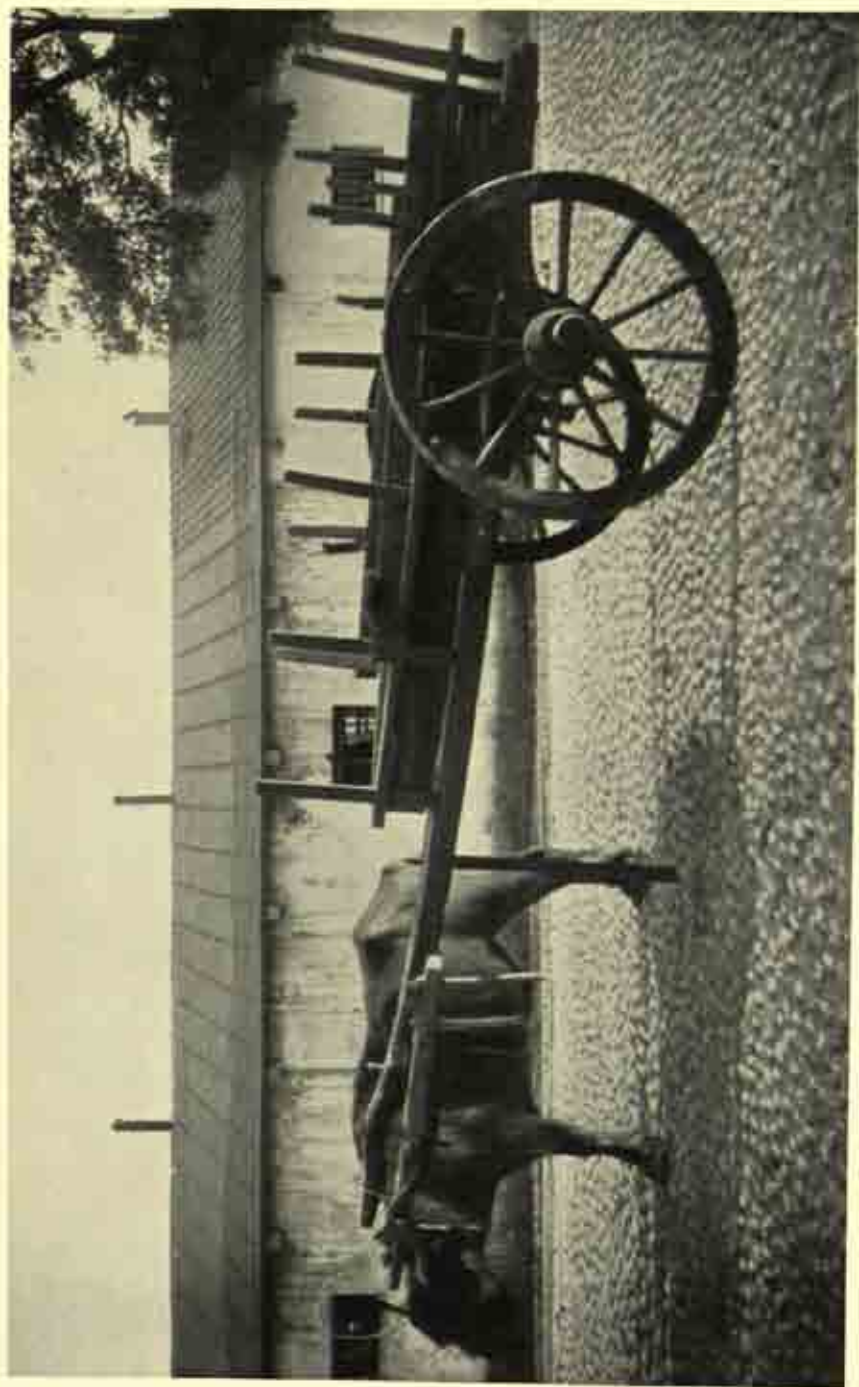


PLATE III



FROM COTTON MS. (Tib. B. v. fol. 61), BRITISH MUSEUM  
by permission of the Trustees

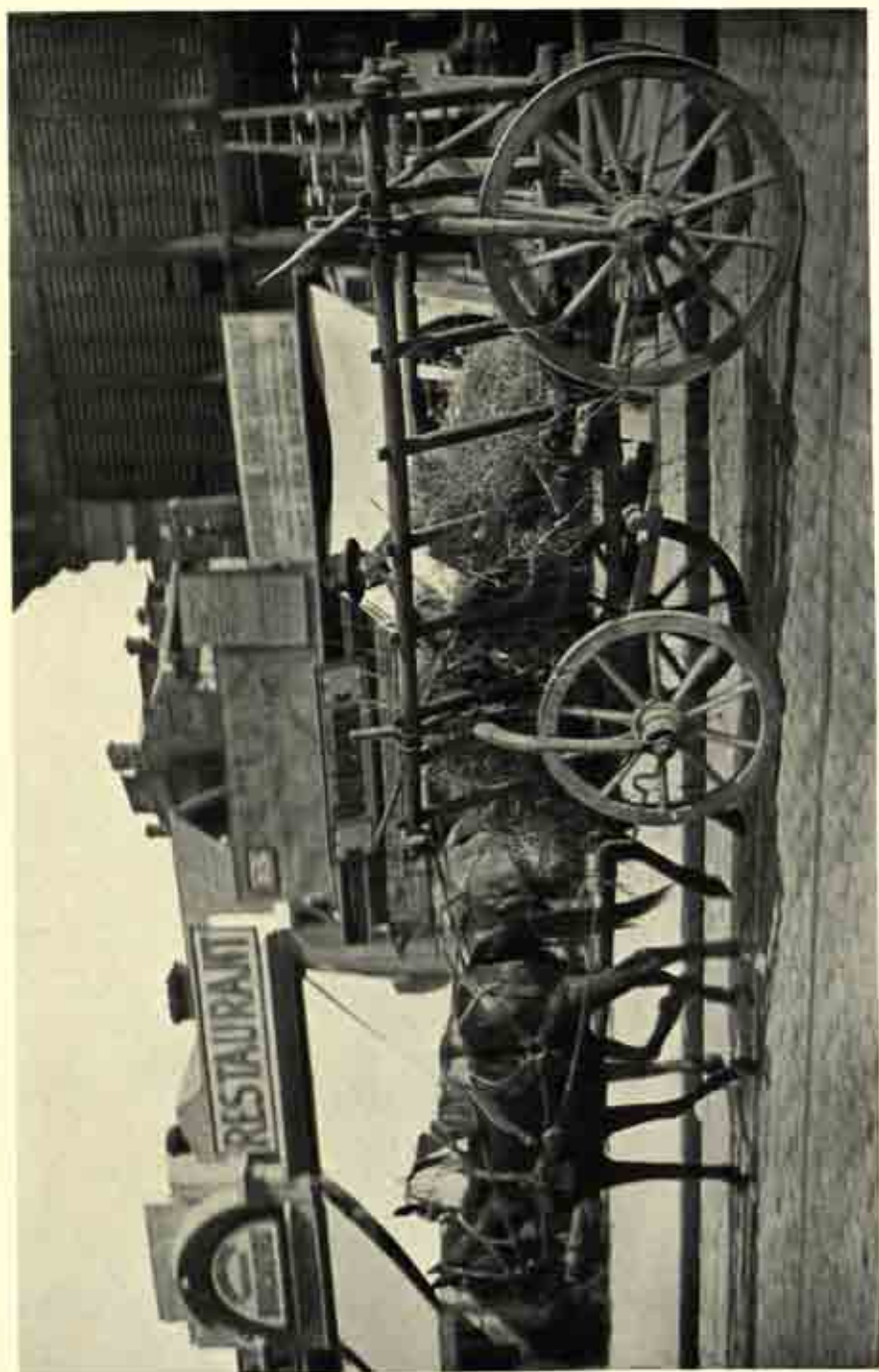
PLATE IV



OX CART, SURHUM, SHOWING YOKÉ, &c.  
Ph. O. G. S. Crawford, 1913

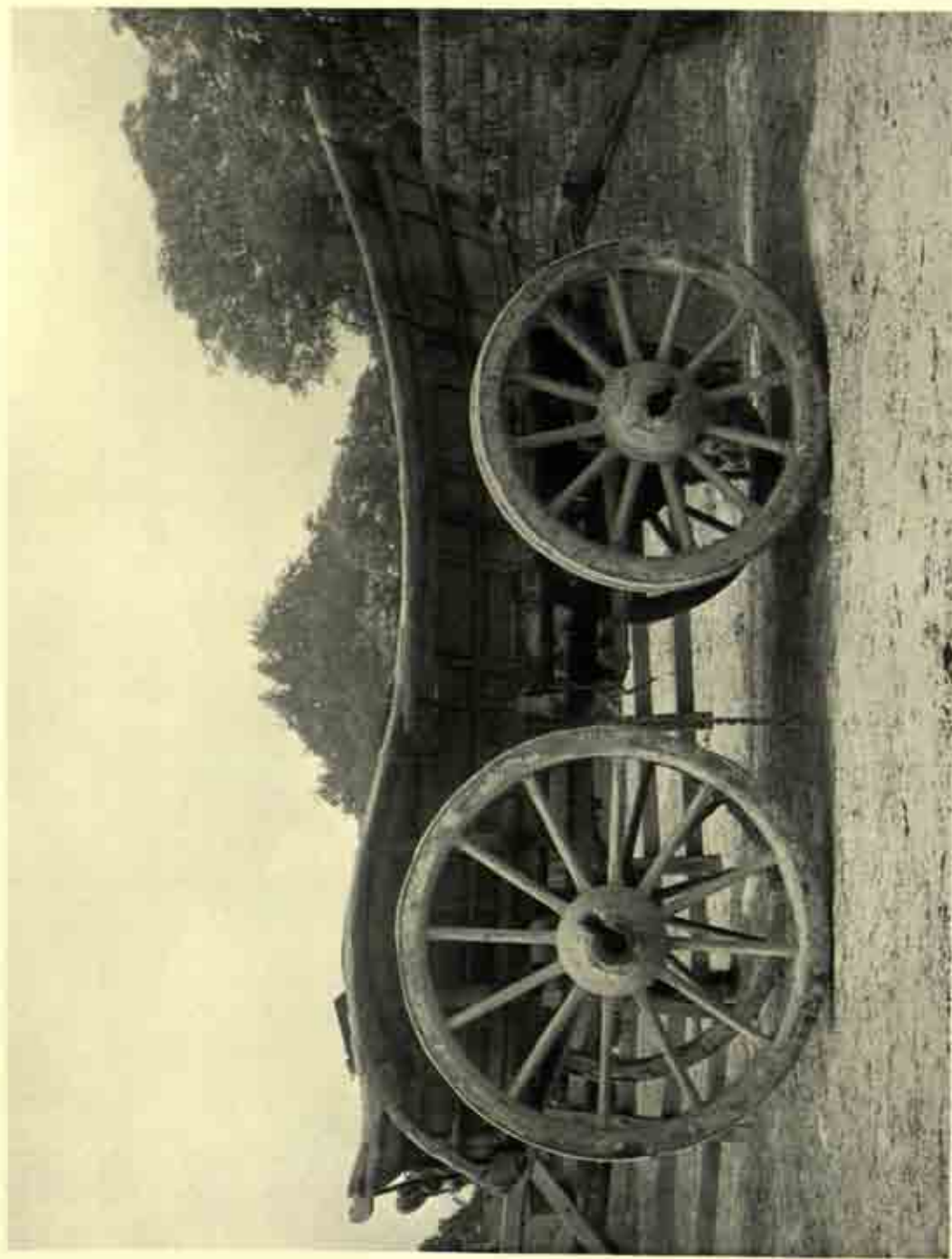


PLATE V



ROMANIAN CART, BUKHAREST  
Pl. O. G. S. Crawford, 1932

PLATE VI

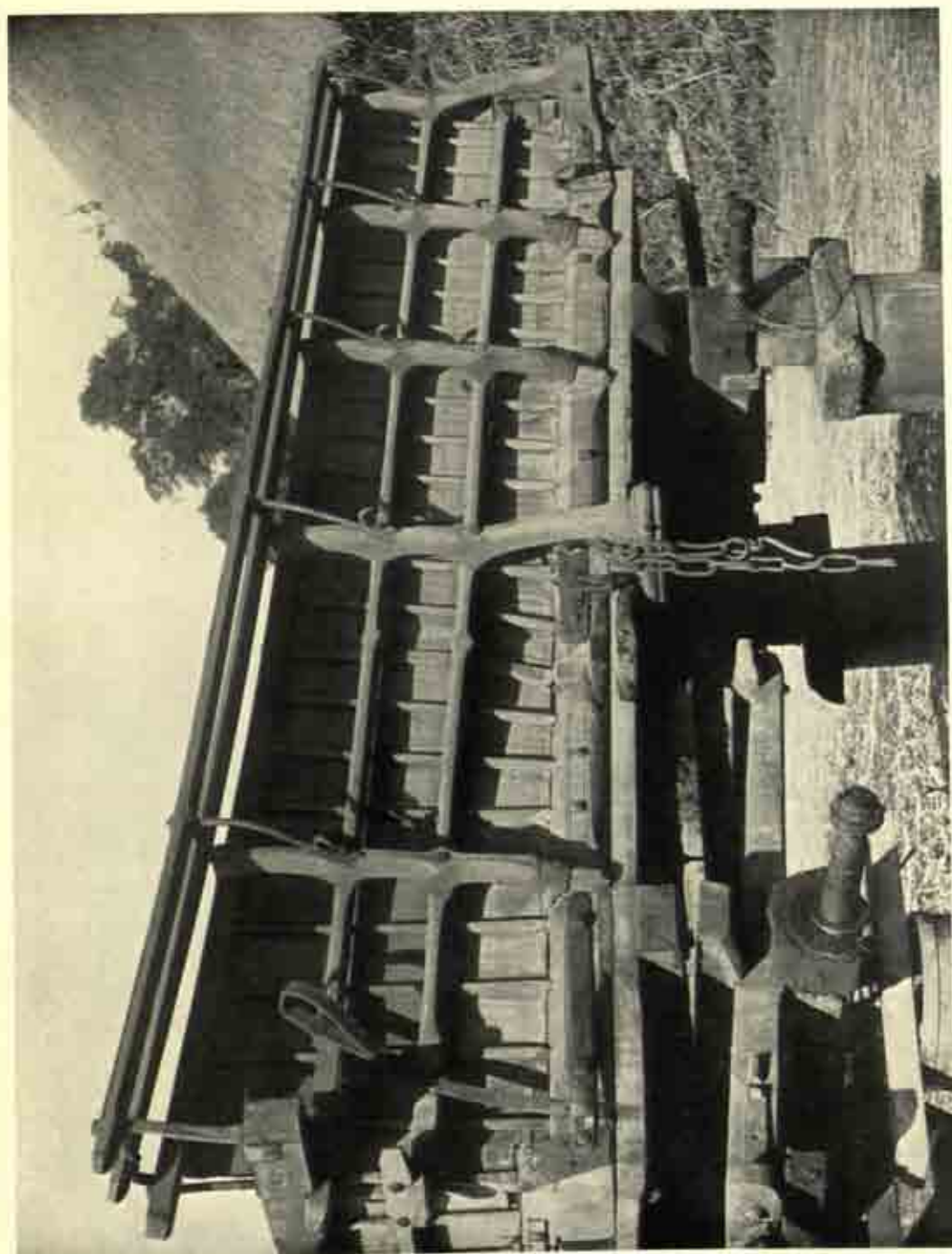


A WILTSHIRE 'HOOP-RAVE' WAGGON

*Ph. R. W. Lane*

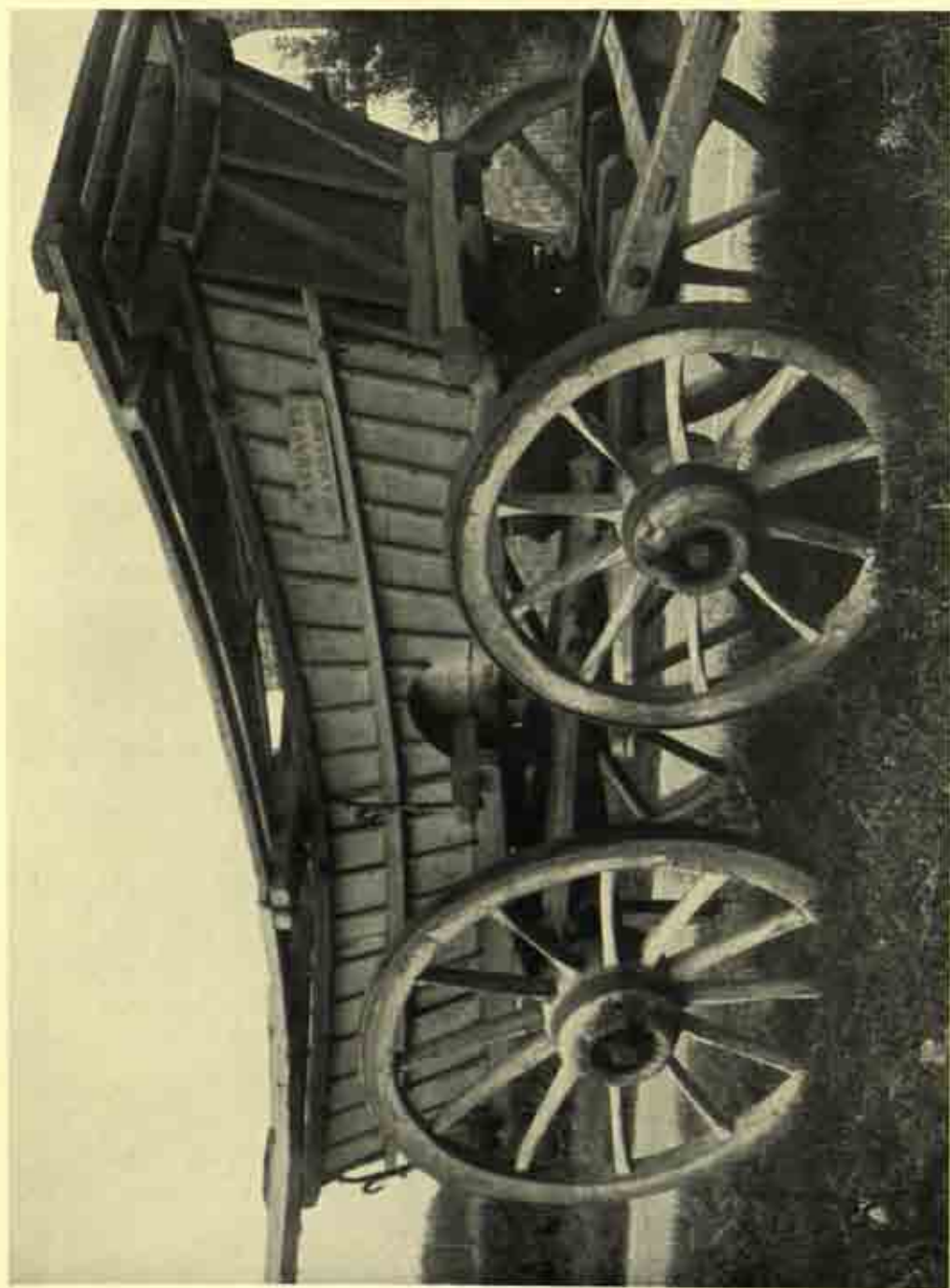


PLATE VII



A HERTFORDSHIRE WAGON  
*Ph. R. H. Lane*

PLATE VIII



[A LINCOLNSHIRE WAGGON  
(note also of wooden axles)]

*Pl. R. H. Lane*



## WAGGONS AND THEIR ANCESTORS

at this time. Probably they were rare, for Henry Best of Elmswell still sent his oats to market on packhorses in 1641. But the late 16th century, and still more the 17th, saw the beginning of those conditions of farming whose spread in the 18th necessitated the general use of waggons.

The skill to make them existed already, for, as Sturt points out, it is in making the wheels that the highest skill is needed; and wheels, similar in essentials to the later ones, were already, as we have seen, being made in the 16th century.

It was not until the 18th century that 'enclosed' farming became the normal method. Between 1727 and 1760, more than 200 private Enclosure Acts were passed, and the size of industrial towns also grew with increased speed. That is to say, the conditions requiring heavier loads became normal, and with them, the use of farm-waggon, for ordinary farming now justified the expenditure of greater capital, as well as providing it. In 1770 Arthur Young includes a waggon, priced at £25, among the desirable implements of a farmer with no more than 70 acres. Marshall at the end of the 18th century noticed that in Warwickshire the harness of the farmers' teams was often ridiculously ornamented, and their horses over-fed and under-worked to save their looks. Such farmers would be likely to take a pride in their waggons too. They could afford to pay for skilled labour, for good material, and for the beautifully shaped and shaved timbers, which were, as Sturt tells us, really an economy, as well as for a little beading and scalloping, which was, from the materialist's point of view, useless. It is rather difficult to share Marshall's indignation!

Somewhere in the 17th or 18th centuries the farm-waggon developed its characteristic surboards. The stage-waggon seems to have had none. This is natural, for the boxes and bales that they carried were usually heavy, rather than bulky. But corn, hay and straw, which formed a large part of the farmers' wares, were light in proportion to their bulk. Surboards, then, were added to farm-waggon, to provide the loading surface needed to carry the full load of some three tons. Further, corn, hay, and straw are apt to spread, so both sides and surboards needed support. Thus there arose the elaborate systems of struts and staffs, which are characteristic of the waggons in PLATES VI, VII, and VIII, as well as of those illustrating Sir Cyril Fox's article.

PLATE V shows another way of tackling the support problem. This 'outrigger' device is in use today, over a large area of Europe, as Mr Crawford's photographs show.



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Its purpose is clearly to help the axle, which looks rather slender, by applying some of the load to the *outer* end of it, as well as to support the body. Iron rods to both fore- and hind-axles apply some of the pull also to the *outer* end. Although the construction is rough, some skill must be needed, in designing the front outrigger, to allow the fore-wheels to pivot, when turning the waggon, as they are clearly meant to do. There are no surboards, but the narrowness of the waggon-body does not suggest that it is meant to carry the heaviest possible loads. Surboards seem to be peculiar to the British Isles. The devices shown in PLATE V are not limited to the area already mentioned, or to the present day. Iron rods taking the pull of the horses to the outer end of the fore-axle appear in a fairly recent photograph from Persia,<sup>9</sup> and it is also to be seen in pictures of English, Flemish, and Italian coaches of the 17th and 18th centuries. The support from the body to the hind-axle appears in a photograph of a Polish waggon in a recent number of the *National Geographic Magazine*.<sup>10</sup>

Little has been said up to now about the development of roads, and there is little to say, except that, at any rate up to the end of the 18th century, they remained as bad as they could be. From 1660 onwards there was incessant legislation on the subject, but it accomplished little. The statements of Defoe, Young, and others are well known, and Malcolm, a Surrey land surveyor, says that even in 1805 he had difficulty in riding six miles in four hours, and for a great part of the distance was in danger of being swamped.<sup>11</sup> The effect of this state of affairs on transport was twofold. It would help to popularize the use of waggons for heavy loads, because four wheels distribute the weight over more ground than two. It also made large wheels desirable, for they move better over rough or heavy surfaces. Malcolm gives 5 ft. 5 ins. as the normal diameter of hind-wheels throughout Surrey, and hind-wheels of similar size are to be seen on existing Wiltshire waggons of the early 19th century. Fore-wheels were smaller, to assist the turning of the waggon—always its weak point. That this was the result of necessity and not choice is clear from the writings of a 19th century coach builder. 'From what has been said about wheels, it is clear that the fore-wheels *should* be as high as the hind ones; but as such a height would interfere with the turning of the coach . . . it is proposed that the fore-wheels

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<sup>9</sup> Plunket, *Europe in the Middle Ages*.

<sup>10</sup> March 1935.

<sup>11</sup> *Agriculture of Surrey*, 1805.



## WAGGONS AND THEIR ANCESTORS

should be of this height' (viz. 46 inches; the italics are mine). Malcolm gives the height of fore-wheels as 4 ft. 2 ins. The waggon-wheel was naturally bigger, but the principle was the same.

With the modern improvements in roads smaller wheels have come into use. By the latter part of the last century the average Wiltshire hind-wheel had shrunk to 4 ft. 10 ins.; the boat-waggon, introduced there about 1900, has hind-wheels of some 4 ft. 2 ins., and the now popular trolley, wheels of about 3 ft. 6 ins. But until the English waggon had reached its 'zenith', its designer could reckon on nothing but bad surfaces on the road.

### REGIONAL WAGGON-TYPES

Sir Cyril Fox has pointed out the need of determining the boundaries within which different types of waggon were or are in use. He says, 'the essential features of the "Glamorgan" waggons though not, I think, so finely developed, occur in Gloucestershire and western Oxfordshire waggons'.

If I am right in interpreting him to mean especially the curved surboards, the panelled sides, and the iron staffs, this type continues into Wiltshire, north Berkshire, and Buckinghamshire. In all these districts many waggons of the type may still be seen, though not, I agree, in so finely developed a form. In particular, the shape of many of the iron staffs is similar, although another shape is also common. I have never seen 'balustered' head- or tail-boards in this area, nor the elaborate shaft-bracings of the 'Glamorgan' waggon, though I have seen some like them in Lincolnshire. Some of the Wilts, Berks, and Bucks waggons have a 'waist' or contraction of the body, to give the front wheels more room to turn. This feature is found on very old waggons, the waggon being then called 'half-bedded'.

Typical measurements in Wiltshire are:—Length of floor, 11 ft. 5 ins.; breadth of floor, 3 ft. 9 ins.; height of waggon in the middle, from the top to the ground, 4 ft. 9 ins.; diameter of fore-wheels, 3 ft. 10 ins.; hind-wheels, 4 ft. 10 ins.; track, 'out-to-out', (*i.e.* the distance between hind-wheels *at ground level*, taking the outside of both wheels) 5 ft. 11 ins.

It is clearly of this type that Marshall writes:—'the Gloucestershire waggon is . . . the best farm-waggon I have seen in the Kingdom . . . the wheels run six inches wider than those of the Yorkshire waggon'. Again: 'the Wiltshire waggons run remarkably wide, full five and a half feet from middle to middle of the ruts. I have measured



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one near six feet from out to out, . . . they are particularly well adapted to a hillside country'.<sup>12</sup>

This width in west-country waggons was one of the considerations which led Brunel to adopt his Broad Gauge (7 ft.) on the Great Western Railway. He wished his railway to be wide enough to accommodate 'easily' road vehicles whose width he puts at 6 ft. 6 ins. This was probably a maximum greater than the average, although waggons 6 ft. 6 ins. from out to out are still to be seen. The northern railway pioneers also based their 4 ft. 8½ in. gauge on the common-road vehicles of their neighbourhood. The railwayman's figure is an inside measurement, and corresponds to a wheelwright's out to out figure of about 5 ft. 1 in. The northern gauge appears to be prehistoric, even in England.<sup>13</sup> But what is the origin of the 'western' waggon track? Of this type (PLATE VI) the eastern limit, as far as I have been able to determine it, is Tring.

The next type (PLATE VII) is found in western Hertfordshire, Bedfordshire, and Northamptonshire. It is remarkably different. It has a flat, or almost flat, line from end to end. The staffs as well as the struts are mainly composed of wood. The sides are ladder-like, and have boarding on the inside, but they are not panelled. The headboard is detachable, as well as the tailboard. The owner of this waggon told me that it was at least 50 years old, and that it had been the usual type round Ware and Hertford. It must be rare in that district now, for it required searches covering three years to find this specimen. (There are still many of them in Northamptonshire.) It was in 'good running order', and three of its wheels had been sent to the wheelwright at the end of the harvest, a practice for which Sturt pleaded in vain. Their absence gave an exceptionally clear view of the construction of the waggon, though depriving it of much of its beauty. A clumsy repair has been made to the main timbers of the body. The measurements of this waggon were:—length, 10 ft. 6 ins.; breadth, 4 ft. 3 ins.; height, 5 ft. 4 ins.; fore-wheel, 4 ft. 3 ins.; track, about 6 ft. This type was seen a little south of Stamford, but not north of it.

A third type (PLATE VIII), still common in the Lincolnshire Wolds, is quite different. It has a curved outline, producing a lofty front,

<sup>12</sup> *Rural Economy of Gloucestershire, 1789, and Rural Economy of the Southern Counties, 1798.*

<sup>13</sup> R. E. M. Wheeler, 'A Prehistoric Metropolis', *ANTIQUITY*, June 1932.



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which is often elaborately painted, with the names and addresses of maker and owner, the date, and ornamental scroll-work. The date may be that of a rebuilding, and not of the original construction. Iron staffs are again the rule, quite different from the 'Glamorgan' pattern. The surboards of the waggon shown are attached to cross-bars, and are removable. This arrangement is not uncommon, though in some waggons they are integral. The one illustrated here was described by the wheelwright who was repairing it as a 'real old standard'. He put its age at 100 years, saying that he had recently repaired for further service one bearing the date 1812.

As will be seen, a recess is cut in the side to give the front wheels a little more room when they turn. But perhaps the most interesting point about these waggons is that the great majority of them have wooden axles. This is true of those which seem to be of quite recent make. The wheelwright told me that he had made and fitted several in the last two years, and showed me the worn-out axles which he had taken off. One had caught fire from lack of grease, and was charred nearly through. I was interested to hear him express the opinion that wooden axles were better on 'the land', but iron ones on a hard road. The preference for these wooden axles in Lincolnshire is quite deliberate, and it is by no means wise to dismiss it as mere conservative prejudice. It is shared by many outside Lincolnshire. I have heard it expressed by a carter, who at the same time was anxious to try pneumatic tyres on farm carts. Such a man is no mere 'laudator temporis acti', and his opinion, though possibly mistaken, is, after all, the opinion of an expert.

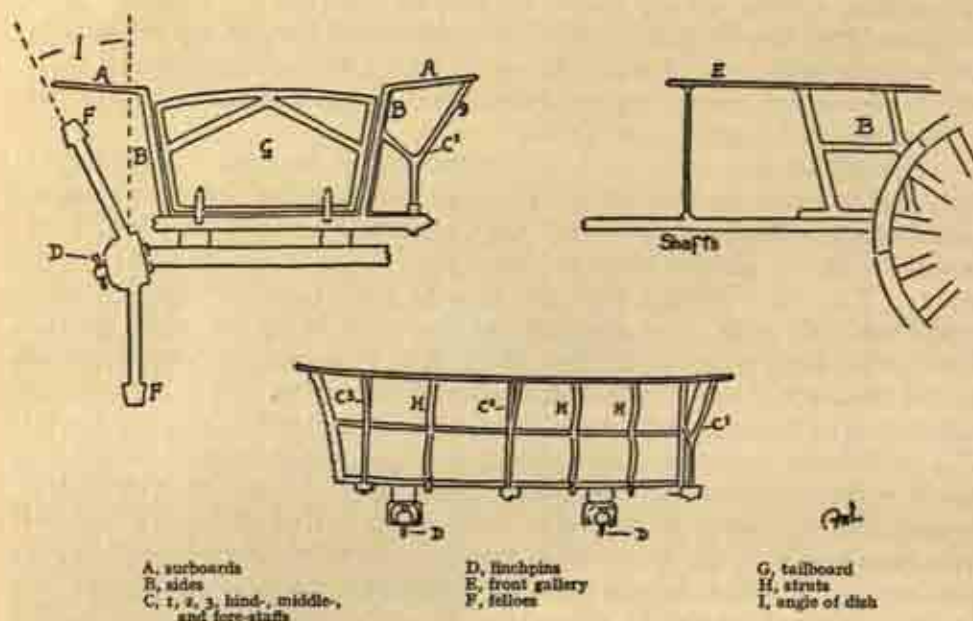
The track from out to out of this waggon is about 5 ft., and it is therefore of just about the same width as those from which originated the 4 ft. 8½ in. railway gauge. Yorkshire waggons, are, as far as I can find out, of similar width. Here we are on the verge of 'the North', historically a region of survivals, so it is appropriate to find here very many of these veterans. They are well kept still, and to the enthusiast, accustomed in the south to grey timbers and battered sides, it is a splendid sight to see them in the harvest-field, blazing under the noon-day sun, in all the glory of primary reds, yellows, and blues.

Measurements (of this specimen), as before: length, 10 ft.; breadth, 3 ft. 8 ins.; height, 5 ft. 2 ins.; fore-wheel, 3 ft. 10 ins.; hind-wheel, 5 ft. 2 ins.; track, about 5 ft.

The choice of suitable terms presents some difficulty. The technicalities of the wheelwright do not, as a rule, solve the problem,

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for they differ in different districts, and are often applied to more than one part of the vehicle. For the moment, those terms whose meaning is not obvious are, I hope, sufficiently defined by the following diagram. The term 'waggon' is used exclusively to mean four-wheeled vehicle, a use which is convenient, and, I believe, correct.



The diagrams are not intended to represent any actual vehicle, nor are details drawn to scale.





PLATE I

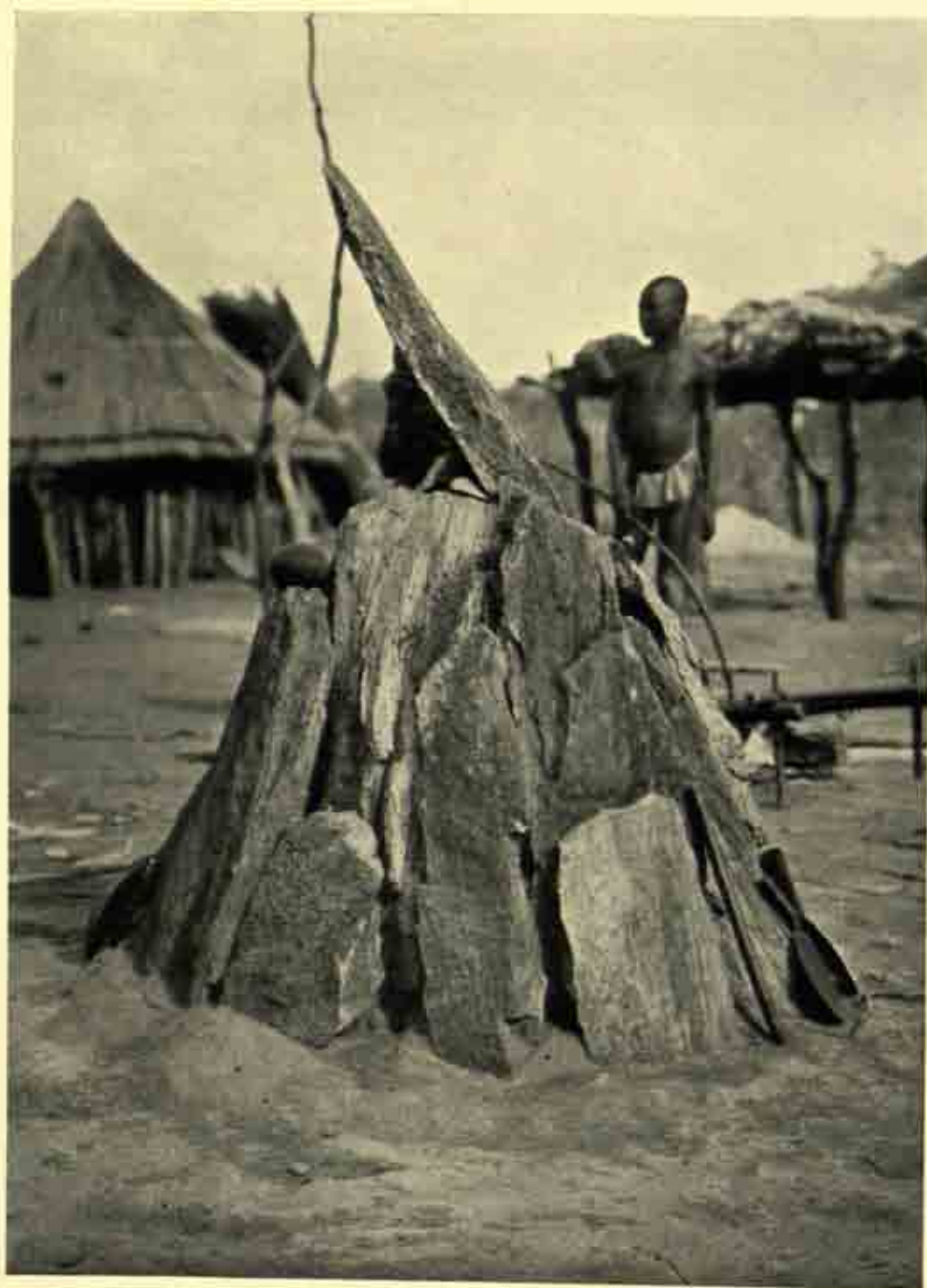


FIG. 2. MORO MISE PYRAMID GRAVE-MONUMENT.  
Figs. 2-5, *PK*; E. E. Evans-Pritchard



# Megalithic Grave-Monuments in the Anglo-Egyptian Sudan and other parts of East Africa

by E. E. EVANS-PRITCHARD

THIS note describes megalithic grave-monuments among the Mise tribe\* to the west of the Nile in Amadi District of Mongalla Province, Anglo-Egyptian Sudan. I visited this tribe in the early part of 1927.

The megalithic monuments described are typical of Mise country. They may be found occasionally among other members of the southern group, the Oggi and Endri, but there possibly they mark the graves of Mise who have died out of their country. The Kederu have probably adopted the heaps of stones and carved wooden posts which they and their neighbours set up over graves. Today these appear to be the predominant types in Kederu country though they are sometimes found in association with pyramids and dolmens which may be the true Kederu forms. It seems likely that they are the true Kederu forms because Kederu speak a dialect identical with that of the Mise, and because dolmens are also found among the Madi who are nearer in speech to the Mise and Kederu than other members of the Moro-Madi language group, among whom, to the best of my knowledge, pyramids and dolmens have not been recorded. Moreover Emin Pasha recorded both pyramid and grave-monuments among the Kederu half a century ago.

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\* The Mise are generally known as Moro but this term has really a geographical rather than an ethnological significance and, as is so often the case in Africa, has been applied to several tribes speaking unrelated languages and with distinct cultures. Of the tribes commonly referred to as Moro the northern group (Moro Kodo, Nyamusa, Biti, and Wira) speak dialects belonging to the Bongo-Mittu group of languages, while the southern group (Moro Mise, Moro Kederu, Moro Endri, and Moro Oggi) speak a dialect of the Moro-Madi group of languages which includes also the Madi, the Kaliko, the Abukaya, the Logo, the Lendu, and the Lugbwara. I may refer the reader to Professor and Mrs Seligman's *Pagan Tribes of the Nilotic Sudan* for further information about the ethnology of this area.

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The Mise monuments have been mentioned by a number of travellers whose statements are transcribed in this paper. The account by Wilson and Felkin was accompanied by drawings but, as far as I am aware, the only photographs of the megaliths which have been published were taken by myself.<sup>1</sup> Most of my knowledge is contained in the photographs and I can add little by way of commentary. I have recorded elsewhere what is known about the grave-monuments of neighbouring peoples to the west of the Nile in descriptions of their customs and beliefs in general.<sup>2</sup>



FIG. 1. MADI GRAVES

(from 'Uganda and the Egyptian Soudan', by C. T. Wilson and R. W. Felkin, 1882, II, 123)

Felkin writes of the 'Lorus', evidently the Moro Mise, of the neighbourhood of Zanga and Kadru, that they have megalithic monuments. 'No signs of life were visible, and the absence of large trees

<sup>1</sup> C. G. Seligman, *Ency. Brit.*, 14th ed., article 'Africa: Archaeology and Antiquities', and C. G. and B. Z. Seligman, *Pagan Tribes of the Nilotic Sudan*, 1932.

<sup>2</sup> 'The Bongo', *Sudan Notes and Records*, 1929; 'The Mberidi and Mbegumba of the Bahr-el-Ghazel', *Sudan Notes and Records*, 1931; and 'Notes on the Bongo-Mittu group of peoples' (to appear in the same journal).



PLATE II



FIG. 3. MORO MISE PYRAMID GRAVE-MONUMENT

PLATE III

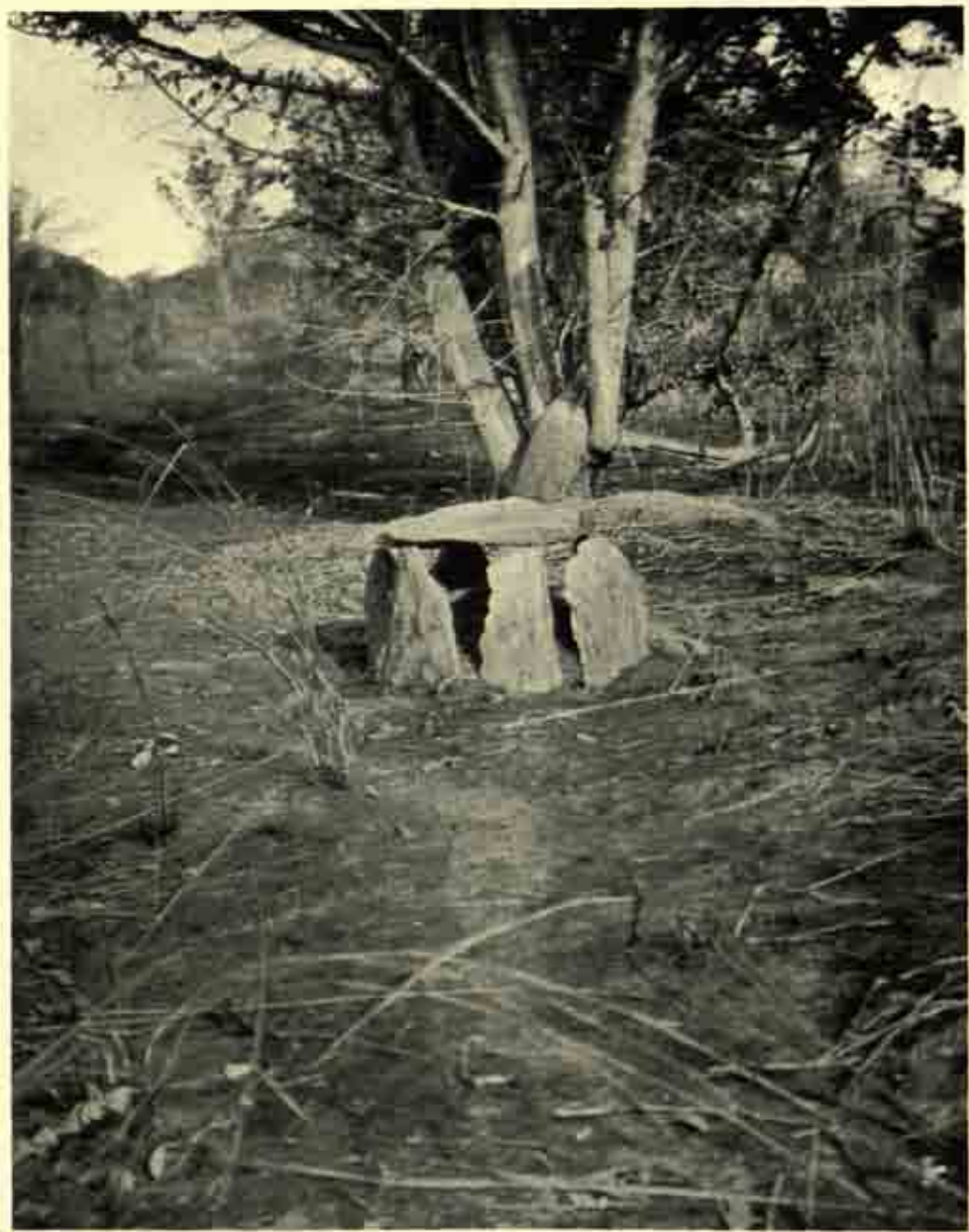


FIG. 4. MORO MISK DOLMEN GRAVE-MONUMENT



PLATE IV

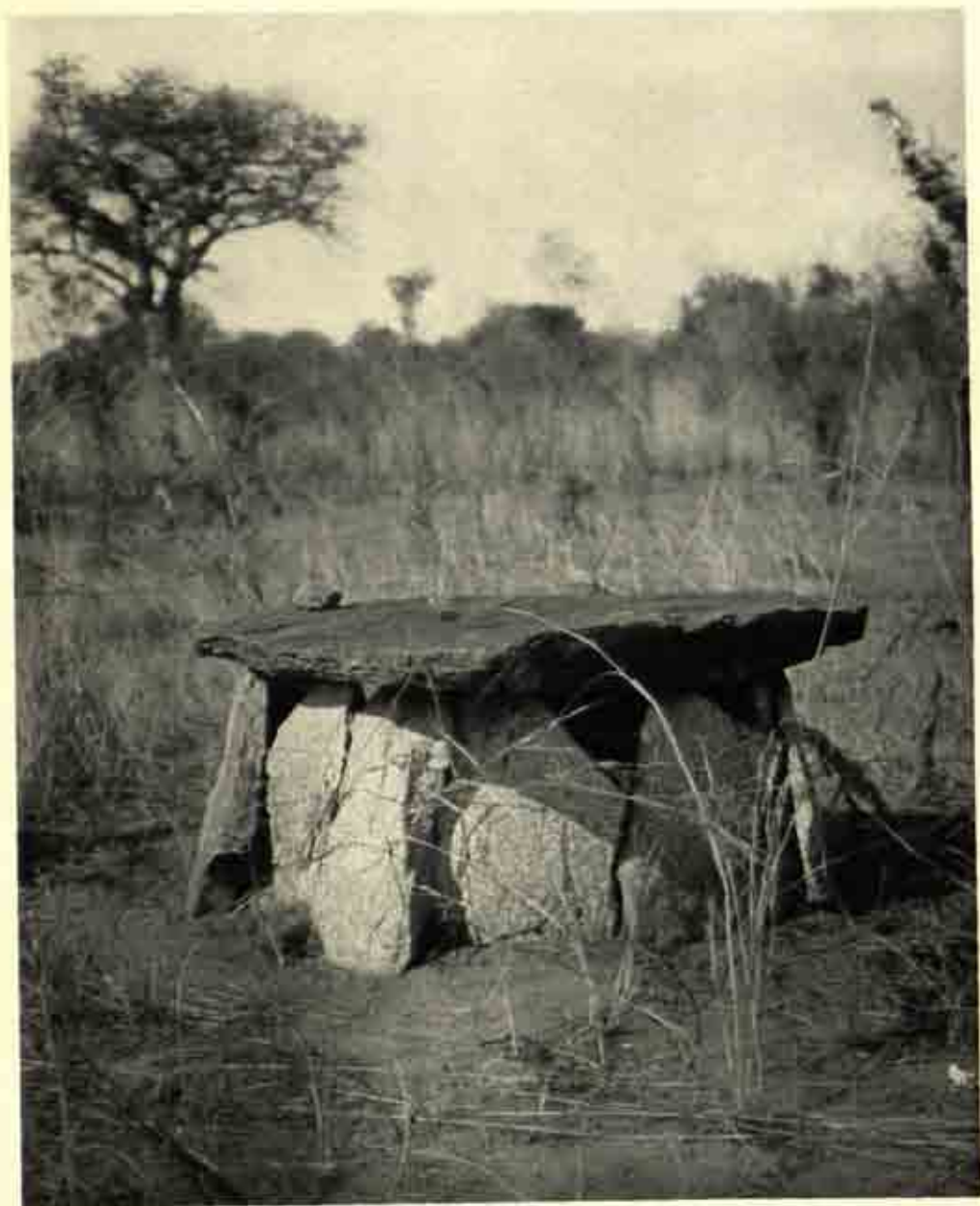


FIG. 5. MORO MISI DOLMEN GRAVE-MONUMENT

PLATE V



FIG. 6. NDIKA OF THE RUVUMA COUNTRY.

FR. ROBERT WEBB

*By courtesy of the Universities' Mission to Central Africa*



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was remarkable ; the ground was sandy, and contained a good deal of iron-stone. Six small streams were crossed, none being of any importance. We noticed a great number of graves on the road, all of which were marked by stones ; sometimes the stones were placed so as to form a kind of pyramid, at other times one flat stone was put upon six or eight others placed in a circle'.<sup>3</sup>



The drawings, reproduced opposite, which accompany Wilson and Felkin's account, are entitled Madi graves, a most unfortunate nomenclature since the people of this area are Mise and not Madi. As the Mise speak a dialect of the Madi language this might seem a

<sup>3</sup> C. T. Wilson and R. W. Felkin, *Uganda and the Egyptian Soudan*, 1882, vol. II, p. 123.

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futile objection if it were not that Wilson and Felkin describe the people living still further to the north, and speaking dialects of a different language, as Madi also, and it is from this people that their Madi vocabulary was taken.<sup>4</sup>

Indeed it is often far from clear about which people Wilson and Felkin are writing. Elsewhere Felkin speaks of the 'Madi or Moro tribe' who live in a tract of country situated Lat. 50° N, Long. 30° 20' E, of which the chief town is said to be Bengue. In speaking of their graves he mentions 'a conical mound of earth, three or four feet high, which is stabbed by flat stones'.<sup>5</sup> It is difficult to know precisely what is meant by 'stabbed by flat stones'.

Speaking of the Kederu, Emin Pasha wrote: 'The village of Mollo is situated near this *chor* (Labikko), in the midst of corn-fields. I noticed there many trees hung with skulls and horns. Unfortunately these trophies were obtained only from dwarf antelopes and beasts of prey, which appear to abound in the vicinity. From a lofty pole in the centre of the village the skin of a python fluttered in the wind. The numerous graves round the village could be recognized by pyramids formed of slabs of rocks or by huge heaps of stones, from the centre of which rose a pole, with three notches in its upper extremity, which were shaped exactly like those which are seen on the ancient Mohammedan tomb-stones in southern Arabia. I saw occasionally a third form of grave enclosed by slabs of stone arranged in a circle, so as to support a larger slab, thus forming a kind of table. The care which the Mittu and Kederu devote to their graves constitutes a fine trait in the character of these tribes, who differ in this respect very favourably from the Bari and Dinka'.<sup>6</sup>

Stigand wrote of the Mise: 'On the march to Saruru stones and rocks are plentifully strewn about, and these are used to mark the sites of graves, of which there appear to be numbers in every direction. The most frequent design is a flat rock, placed like a table, resting on four or six long stones planted upright in the ground. Another arrangement is a small ring of upright stones like a miniature Stonehenge. As such pigmy monuments are practically unaffected by time or weather, it is not surprising that in course of time the whole country should be covered

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<sup>4</sup> Id. vol. 1, pp. 352-4 and 'Notes on the Madi and Moru Tribe of Central Africa', by Robert W. Felkin. *Proc. of the Royal Soc. of Edinburgh*, 1883-84, XII, 303-53.

<sup>5</sup> *Proc. of the Royal Soc. of Edinburgh*, 1883-84, p. 327.

<sup>6</sup> *Emin Pasha in Central Africa*, 1888, pp. 356-7.



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with them. I do not remember seeing anything of the same sort anywhere else in Africa; the *Madi* kinsmen of the *Moru* do nothing to mark the site of a grave'.<sup>7</sup>

From the descriptions of Wilson and Felkin, Emin Pasha, and Stigand, it appears that there are at least two types of grave-monuments found among the *Mise* and the *Kederu*. We may refer to them as the pyramid type (since it has been so described by previous writers) and the dolmen type.

The structure of the pyramid type of grave in *Mise* country is evident from the photographs (FIGS. 2-3) which accompany this paper, so that only a short verbal description is required. They consist of slabs of flat white granite leaning together in pairs one above another with one slab much taller than the rest pointing either to the rising or to the setting sun. If the grave contains a man the stone pointer leans to the east, whereas if it houses a woman it points to the west; because when a man rises from sleep he shades his eyes and looks to the rising sun which marks the beginning of a hunter's day; whereas when a woman wakes from sleep in the afternoon, she shades her eyes to the west, for when the sun goes down in the west she prepares the evening meal.

Some of the monuments I saw had three central stones leaning against each other, one of them being considerably taller than the other two and pointing east or west, and against these three central stones were propped many smaller slabs of granite. Others consisted of two large slabs, one being the pointer and overlapping the other and leaning against it. Against these two slabs were propped two other large slabs pointing north and south. Other and smaller slabs were placed at varying angles and in haphazard fashion around these four central stones.

The pyramids are generally taller than a man and some must be over ten feet in height. The longest stone pointer I have seen is shown in FIG. 3. It marks the grave of *Beliyiye*, father of Chief *Yilu*, at *Lui*. The other photographs of pyramids are all of women's graves.

Enclosed within these high pyramids one sees only a small heap of earth, but there are also graves which consist of a large earth mound with flagstones covering it so as to resemble a sloping pavement. In these graves the stones are supported by the heap of earth on which they lie and there is not always a pointer.

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<sup>7</sup> Major C. H. Stigand, O.B.E., *Equatoria, the Lado Enclave*, 1923, pp. 51-2.



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Among the Mise one observes many small pyramids about two feet high built in the same manner as the larger pyramids but on a smaller scale. These mark the graves of children. One also sees small mounds of earth uncovered by stones just outside huts. These are graves of infants. Still-born infants have three small pegs driven into the ground above their grave. All graves are dug within the homestead enclosure.

The dolmen type of grave consists of a ring of generally three, four, or five, upright stones with a large slab placed over them so as to give the appearance of a table. Their form is clear from the accompanying photographs (FIGS. 4, 5). I have never seen the small ring of upright stones described by Stigand, nor has Dr Fraser, of the Church Missionary Society Mission in Mise country, ever seen them. Possibly the surmounting slab had in the case of some dolmens been knocked over by elephants.

This type of grave is used for first-born children. The grave of a boy has three and that of a girl four stones. There are sometimes more than four stones but Dr Fraser told me that it is only the number three or four which is significant of the sex of the deceased. If a first-born child dies very young a dolmen is erected over his grave and the next child, having a peculiar social status, will, on his death, have erected over his grave a peculiar monument consisting of a heap of stones with a large slab of granite forming an upright pointer in the centre. I visited a newly-erected monument of this kind. It consisted of a huge heap of stones some ten feet high, in which was placed an upright slab of granite facing though not pointing to the east. It appears also (though I am not sure of this) that it is sometimes the first child to die who may be commemorated by a dolmen grave.

Stigand's statement that this is the most frequent type of grave is misleading. Admittedly the number of graves of this type that one sees in the bush is numerically out of all proportion to the presumed ratio of first-born children to other children born, and indeed it may be more frequently seen there than the pyramid type of grave. But in the neighbourhood of villages, graves of the pyramid type far out-number those of the dolmen type.

The dolmen type is likely to prove more durable, indeed it is almost indestructible; whereas the pyramid type may be upset in course of time. The earth mound which supports the smaller stones may be washed away by heavy rains, or the level of the ground may sink since there is a grave below, or the stones may be knocked down by elephants



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and possibly by other large beasts. In the dolmen type, on the other hand, the upright stones are protected from rain by a flat stone placed over them. Nevertheless natives say that they are sometimes knocked over by elephants.

I have seen pyramidal constructions in the bush in all stages of decay. Sometimes only the earth mound has been washed away while sometimes all the slabs of granite have fallen to the ground. How many years usually elapse before the stones fall I cannot say, but my impression is that they fall much more rapidly than one would suppose judging from the state of many of those in villages vacated only a few years ago. However, they probably outlast the memory of the man or woman whose resting place they mark and the stones may then be taken away for the construction of new graves. On the other hand, all the dolmen graves I have seen have much the same appearance, whether they are new or old.

In some parts of the country flags of granite are found in a natural state having split off from the rock outcrops, as for instance in the neighbourhood of Lanya, whereas in other parts of the country they have to be blasted, as at Galia. Wood is piled up on an outcrop of rock and a large fire kindled. When the fire is at its fiercest cold water is poured onto the heated rock, which splits along the line of water flow. By this means it is possible to obtain granite slabs of the size required for the construction of pyramids.

Stigand's statement that 'the Madi kinsmen of the Moro do nothing to mark the site of a grave' is undoubtedly incorrect for we have several descriptions of megalithic monuments and a photograph of a dolmen from Madi country.

Emin Pasha refers to Madi grave-monuments in two passages but he describes a form different from those found in Mise country. 'The night before our arrival a leopard had made its way into the little village and killed a man, consequently we had the opportunity of witnessing a funeral. A circular hole, about three feet in diameter, and about five feet deep, was dug in front of one of the huts and the corpse, clothed in a skin, was placed in it in a squatting position, with arms and legs drawn up. Then earth was shovelled in and stamped firmly down, and the grave was covered with stone slabs.\* The second passage runs as follows:—'In the middle of the village, which consists of the usual small Madi huts, infested by bugs, the mother of the reigning chief is buried; a flat stone and several long poles, on which hang

\* *Op. cit.* p. 260.



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amulets, mark the grave. The father of the chief is buried at the entrance of the village, his grave being marked by an upright stone about seven feet high, and a post notched at its upper end'.<sup>9</sup>

A photograph of a dolmen grave-monument which accompanies an article on Madi rain-stones by Mr F. H. Rogers is said to be a photograph of graves of former rain-chiefs; it shows a dolmen like those of Mise country (see FIGS. 4-5) and what appears to be a ring of stones without a table surmounting them, like the rings of stones mentioned by Stigand<sup>10</sup>. Mr J. H. Driberg tells me that he has seen a dolmen-like structure, consisting of two uprights and a slab across them, at the village of Kenyi Karalla in Kakwa country in Uganda. The Kakwa are classed as Bari-speaking, and as they are adjacent to the Madi it is possible that they have borrowed dolmens from this people. This is probable because neither the Bari proper nor any of the other Bari-speaking peoples (Shir, Kuku, Mandari, Nyangwara, Fajelu and Nyefu) are known to possess dolmens. Mr Driberg cannot say whether this dolmen-like structure was a grave-monument or not.

Though no further examples of pyramid and dolmen grave-monuments are known in the Anglo-Egyptian Sudan, the miniature dolmen-shrines present at the door of every hut among the Lotuko-speaking Lango of Mongalla Province, to the east of the Nile, must be mentioned. These are called *natifini* and Professor and Mrs Seligman describe them as 'miniature cists about a foot high, resembling rough museum-models of dolmens. They are usually erected at the side of the house soon after it is built, the master of the house being responsible for upkeep. In addition there are, near the entrance of a hamlet, other *natifini*, in which the whole community is interested since communal rites are performed at them'.<sup>11</sup> Colonel Lilley furnished the information that, among the Lango, graves are always to be found close to the stone cists and that 'the body is interred on its side with the feet towards the *natifini*'.<sup>12</sup> It may be that the *natifini* are connected not only with the cult of ghosts but also with a spiritual being called Naijok. Mr Driberg recorded that 'some *natifini* have conical grass roofs over them, and that annual sacrifices are made at the *natifini* to preserve health'.<sup>13</sup> These stone cists cannot be called grave-monuments though they are associated with graves.

<sup>9</sup> *Op. cit.* p. 261.

<sup>10</sup> F. H. Rogers. 'Notes on some Madi Rain-stones', *Man*, 1927, no. 58.

<sup>11</sup> Seligman, *op. cit.* p. 353. Two photographs and a drawing are shown.

<sup>12</sup> *Id.* p. 354. <sup>13</sup> Seligman, *op. cit.* p. 354.



## MEGALITHIC GRAVE-MONUMENTS

The monolithic circles which occur in villages of the Lotuko and some of the Lotuko-speaking tribes have no relation to graves and are quite unlike the pyramids and dolmens of the Mise. The stones are usually small but may be up to three to four feet high. 'These are built at the present day and are squatting places for the men'.<sup>14</sup> A photograph of one of these squatting places among the Lokoia, reproduced in *Pagan Tribes of the Nilotic Sudan*, reminds one strongly of Islamic praying-places in Egypt.

There is a report of dolmens in the Ruvuma country of the diocese of Masasi to which Mr Robert Webb has kindly drawn my attention. Mr Webb tells me that the country from which they are reported lies near the Tanganyika-Portuguese East Africa boundary. These are known as *ndwika* and a native informant knew of three of them. One was described as 'very large and flat, big enough for me to lie on. I slept on it that night. It stands on three legs like a stool'. Of the others this native said: 'there is another *ndwika* on the road to Tunduru but that is not flat and it stands on one stone, not three. And there is still another about half an hour's walk from Lulindi. The little river which crosses the road from Luatala is called *Ndwika* because of the *ndwika* stone which is near to it. The people say that it is the work of spirits. It has been like that for a long, long time. Even the oldest men cannot remember the time when it was not there'.

One of these stones, that near Lulindi, was visited by Miss M. Roe and Miss Gibbons of the Universities' Mission to Central Africa and is shown in FIG. 6, by the courtesy of Mr Webb and the editors of *Central Africa*. Miss Roe described it as follows: 'It is in the forest away from any path. The large flat stone stands not on another stone but on an ant-hill. The top is piled up with little stones, almost certainly put there by people as offerings to the spirits'.<sup>15</sup> It would appear likely that denudation and erosion are responsible for the position of this particular stone and that it is not due to human agency. It is possible, however, that there may be supporting stones round which termites have built.<sup>16</sup>

Dolmens have been discovered in fair profusion in southern Abyssinia by R. P. Azaïs about 150 kilometres southeast of Harar, and

<sup>14</sup> Id. p. 307.

<sup>15</sup> M. Roe, 'Ndwika Stones'. *Central Africa*, May 1930, p. 93.

<sup>16</sup> Mr Webb informs me that authorities at South Kensington and elsewhere, whom he has consulted, do not favour the supposition that the stone rests on top of a termite mound.



menhirs are abundant in various parts of southern Abyssinia.<sup>17</sup> Of the dolmens the Rev. Father Azaïs and M. R. Chambard write 'On peut constater leur parfaite ressemblance avec mégalithes analogues de France qui ne diffèrent pas eux-mêmes des autres dolmens trouvés un peu partout, aussi bien dans les pays scandinaves qu'en Afrique du Nord. Un des dolmens a été fouillé et a fourni, outre des ossements humains, des fragments de poterie, un anneau d'argent et un gros grain de collier de métal massif, très lourd (Octobre 1922). M. Pottier, qui a examiné les photographies envoyées par le P. Azaïs, lesquelles viennent de paraître dans *L'Illustration* (30 Mai 1925), ne pense pas possible d'assigner une date aux dolmens d'Abyssinie. Ils peuvent d'après lui, être beaucoup plus récents que ceux de France, qu'on place à la fin de la période néolithique et au début de la période du bronze, vers le second millénaire avant Jésus-Christ'.<sup>18</sup>

This summary has been quoted to show that while the dolmens of Abyssinia are grave-monuments they are of considerable age and do not appear to be erected by peoples of present-day Ethiopia.<sup>19</sup> It seems that the megalithic grave-monuments of the Mise and Madi are unique in Africa as contemporary structures.

It must be mentioned, however, that Dr Küsters compares Mise dolmens with similar forms in Madagascar: 'Madagaskar bietet mit den Gräbern der Vornehmen eine ähnliche Form. Auch hier werden durch kunstliches Erhitzen und plötzliches Abkühlen grosse Steinplatten abgesprengt, diese in Kammerform zusammengestellt und die "Grabmutter", eine besonders grosse Platte, darüber gelegt'.<sup>20</sup>

The Editor wishes to acknowledge with many thanks the help received from Dr C. G. Seligman and from the Royal Anthropological Institute, during the author's absence in the Anglo-Egyptian Sudan.

<sup>17</sup> R. P. Azaïs et R. Chambard, *Cinq Années de Recherches archéologiques en Ethiopie*, 1931. See also A. Kammerer, *Essai sur l'Histoire antique d'Abyssinie*, 1926, and *Le Mer Rouge l'Abyssinie et l'Arabe depuis l'Antiquité*, 1929. Some of Father Azaïs' results were previously published in *La Géographie*, July-Aug. 1927, pp. 1-39 under the title of 'Exploration archéologique en Ethiopie méridionale, Mai-Novembre 1926'. See also article by M. E. Pottier in *L'Illustration*, 30 May 1925 and 16 April 1927 and an article of M. Ravaisse in *La France Illustrée*, 27 October 1923.

<sup>18</sup> R. P. Azaïs et R. Chambard, *op. cit.* pp. 172-3.

<sup>19</sup> Amongst the many megalithic monuments of Abyssinia are some phallic menhirs (see Azaïs, plates 69-80). It may be worth recording that phallic menhirs have also been reported from the western (French) Sudan, on the plateau of Tondidaro, region of Niafunke (see Octobon, *Statue-Menhirs*, p. 558). EDITOR.

<sup>20</sup> P. M. Küsters, 'Das Grab der Afrikaner', *Anthropos*, XIV-XV, 1919-20.







PLASTER PAINTING OF GESE FROM A MEYDUM MASTABA

Cairo Museum

(See *Antiquaire*, March 1935, p. 121)



# Pyramids and their Purpose\*

## II. THE PYRAMID OF KHUFU (THE GREAT PYRAMID)

by NOEL F. WHEELER

*Late Field Director, Harvard-Boston Expedition*

AS measurements play a certain part in this article it is as well, before dealing with the pyramid of Khufu, to consider the ancient Egyptian measures.

Different values of the cubit or ell (referred to as E), are found in different sites, and in different structures of the same site and period; but it seems quite certain that the Pyramid Age ell had a value between 20.58 and 20.63 in. In later times the variation is much wider. Opinions differ on the division of the ell, since there was also a 'short ell' of 6 palms or hands (referred to as H), whereas the normal ell was divided into 7 hands. Most authorities take the ell of 20.58-20.63 in., divided into 7 hands of 2.94-2.95 in. each; and for the short ell, 6 hands of the same value, giving the value of the short ell as 17.64-17.68 in. But Junker (*Giza I*) gives a normal ell of 20.64 in., divided into 6 hands of 3.44 in. each; this last is not quoted elsewhere. Petrie (*Ancient Egypt*, 1930) gives good reasons for believing that the normal ell was used in the pyramid of Khufu and most other places, but that the pyramid of Khafre (2nd pyramid) and the South Pyramid at Dahshur utilized the short ell of 17.656-17.76 in. In the table of measurements of the pyramid of Khufu (see pp. 183-5), the average value of the ell is 20.614 in., and the division into 7 hands produces nearly double the number of cases of whole numbers of ells compared with the 6-hand division; so that it may safely be assumed that the former is the one used by the builders.

The hand was divided into 4 fingers or digits, each of .75 in. There were also in use, according to Borchardt, a large span of 10.35 in. and a small span of 8.59 in.

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\* The first article of this series was printed in the March number, pp. 5-21.

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A table of the values of the ell, found or deduced by various authorities at various sites, is given on page 183.

There is a cubit measure, of the New Kingdom, which measures 20.59 in., in the Cairo Museum (no. 452).

Measurements of the Great Pyramid—its passages, galleries and chambers—will be found in detail on pages 183-5.

### THE PYRAMID OF KHUFU (FIGS. 1-4)

This pyramid stands on a rock-plateau forming the eastern edge of the Libyan desert, about 5 miles from the village of Giza, from which it gets its modern locality-name. The rock-plateau is approximately 100 feet above the cultivated plain and 197 feet above sea-level.

It is built on a square base, of which the sides differ in length by only about an inch; the average side is 755 ft. 9 in. (440 E), the height above the base platform is 481 ft. 10 in. (280 E), or rather *was* this height originally. No further measurements will be given here in other than ancient Egyptian ells, hands and fingers.

The core masonry is built in level courses throughout of limestone blocks, and the whole was cased in fine white limestone from the ancient quarries near what is now Turrah, on the east side of the Nile, 12 miles from the Pyramids. After a few years' exposure this limestone mellows to a cream colour.

The original entrance is in the north face some 30 E above the base, and is at the upper end of a long passage descending at an angle of about 26 degrees through the core of the pyramid and into the rock for a total length of 220 E. At its foot, after a short horizontal stretch, is a rough unfinished rock-hewn chamber. From the roof of the Descending Passage, at about 50 E from the entrance, rises the Ascending Passage, also at an angle of about 26 degrees. This Ascending Passage opens out at a little less than half its floor-length into the Grand Gallery, their floors being continuous in the same line, to the centre-line of the pyramid; from this point there is a short horizontal passage passing through the Portcullis Chamber into the King's Chamber. From the junction of the Ascending Passage with the Grand Gallery a horizontal passage leads to the Queen's Chamber, which is on the pyramid centre-line. From this same junction a wandering well-shaft descends, vertically in places, to an opening in the west side of the Descending Passage near its foot.



## PYRAMIDS AND THEIR PURPOSE

### DESCENDING PASSAGE (FIGS. 1-2)

This resembles the usual entrance-passage and leads to what in other pyramids is the one and only burial-chamber cut in the rock. It differs from others only in that the burial-chamber is unfinished, that the ascending passage leads off it, and that the well-shaft opens into it. In the small horizontal passage at its foot, just short of the chamber-entrance, is an unfinished enlargement which gives the impression of being an embryo portcullis recess ; and in the chamber is an unfinished pit, and a blind extension of the horizontal passage for a distance of over 30 E. The angle at which the Descending Passage slopes is quite usual, that of the Third Pyramid and of one of the small Queens' Pyramids being within  $\frac{1}{2}$  a degree of it. The passages of the Giza, Dahshur and Meydum pyramids are all between 21 and 34 degrees.

### ASCENDING PASSAGE (FIGS. 1-2)

This passage, which has the same cross-section as the Descending Passage (2 E, 2 H high ; 2 E, 1 F wide), has its entrance in the roof of the latter passage. This opening was concealed originally by the insertion of a single block of limestone which formed an indistinguishable part of the passage roof. It remained in place until the plundering of Caliph Al Mamoun in A.D. 820, when it fell, and the sound of its falling led his excavators to tunnel into the Descending Passage. Immediately above the position of this fallen stone are now three granite plug-blocks, with a thin film of plaster between their faces and the passage which they otherwise exactly fit, and similar plaster between the blocks.

Above this point the passage has no special features except the three Girdle Stones, which are part of the masonry forming the four faces of the passage, but are of granite, while the normal passage masonry is of limestone. The roof of the passage ends with the beginning of the Grand Gallery, the floor continues into the gallery and ends at a point perpendicularly opposite to the termination of the roof.

### GRAND GALLERY (FIGS. 3-4)

This is the largest individual space in the pyramid, some 91 E long, its floor being a direct continuation of that of the Ascending Passage, but the height extended to 16 E. The side and end walls taper inwards as they rise, at about 20 on 1. This slope, or 'batter', is that used in a number of enclosure walls of temples, etc., and is attained in the gallery by overlapping the courses, 7 overlaps in the upper end wall and

# **PYRAMID OF KHUFU.** **Chambers & Passage System** *(Granite is shaded, remainder is Limestone)*

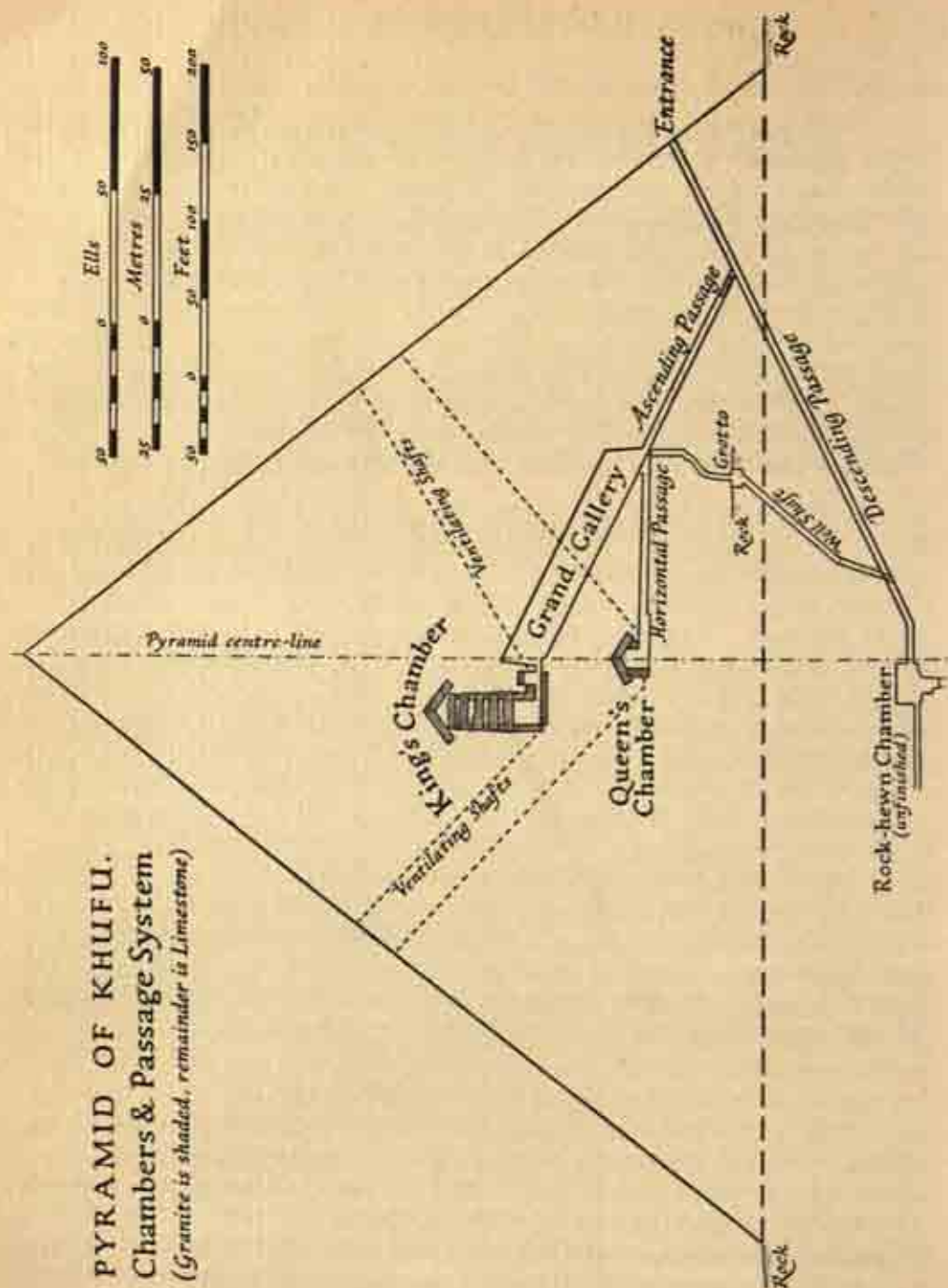


FIG. 1



# PASSAGE SYSTEM

Construction as left by the builders and practically as seen today (indicated in black).

*Presumed construction originally intended before burial (solid red) and after burial (hatched red)*

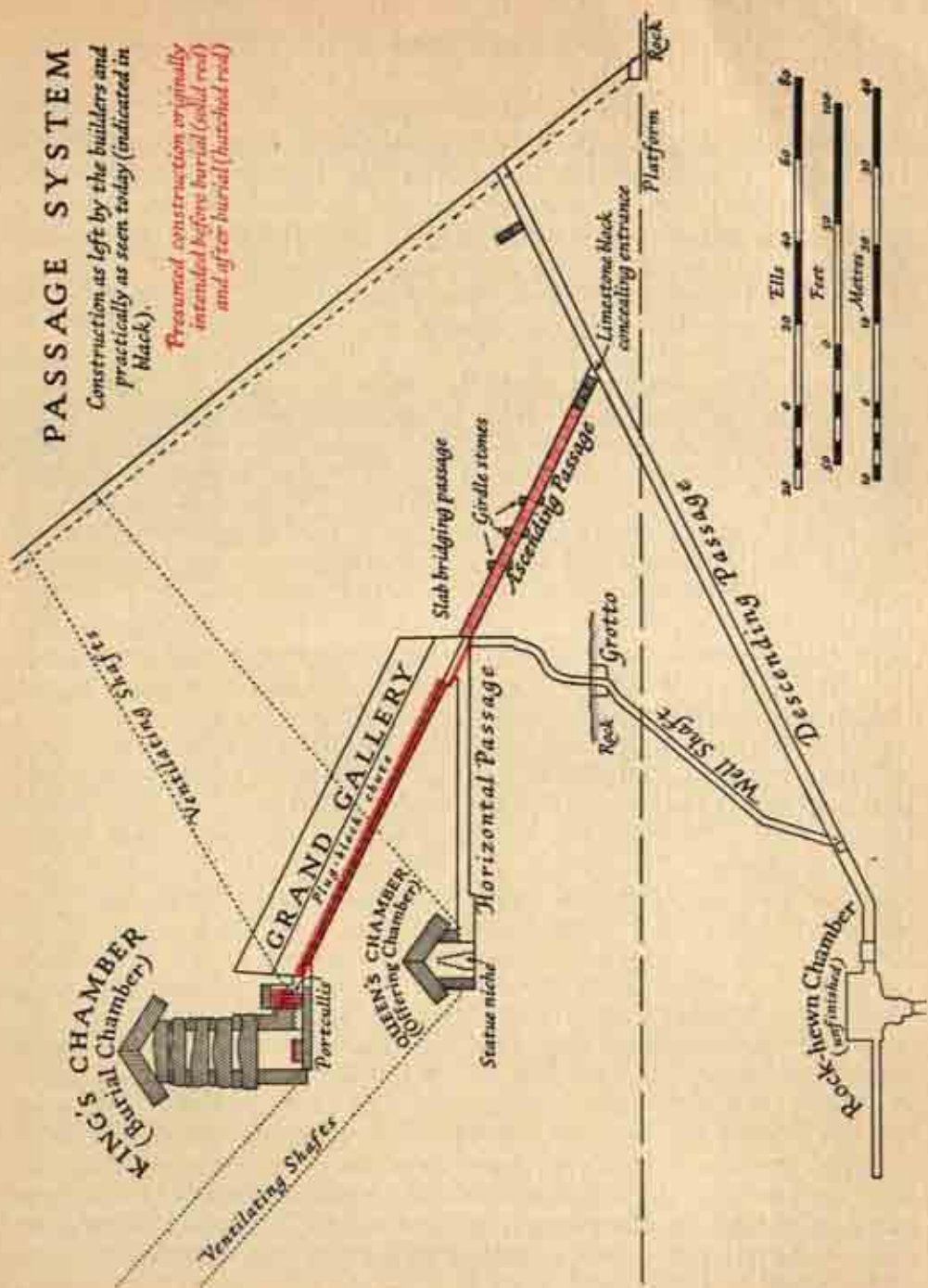


FIG. 2

## ANTIQUITY

6 in the lower. The roof stones are similarly overlapped, the slope of the gallery itself being nearly 1 on 2 and the slope of each roof block 1 on 2.2. The same method of overlapping is found in the statue-niche in the Queen's Chamber, and was no doubt intended, like the six superimposed ceilings of the King's Chamber, to give rigidity under great load.

The floor of the gallery is made into a chute for the plug-blocks by the addition of a shallow ramp up each side, leaving a channel in the centre of the same width as the Ascending Passage. In the upper surface of these ramps, at equal distances apart, are 28 sockets. In each side wall, at a vertical height of 8 E and 5 H above the floor (about 16 feet) is a shallow longitudinal groove running the full length of the gallery. At the lower end of the gallery the floor stops short at the drop where the Horizontal Passage leads off to the Queen's Chamber, and at this point there is a recess in the floor for a large slab to rest, bridging the distance to the top end of the Ascending Passage and making one continuous floor therewith. The sockets in the side walls for 5 supporting beams to this 'bridge'-slab are visible. At the upper end, the floor of the continuing passage intrudes horizontally into the gallery for about 3 E, forming what is known as the Step: this, as all the masonry of the Grand Gallery, is of limestone.

The only other notable feature is the entrance of the well-shaft. With the structure complete this shaft was not visible, but is uncovered by the removal of the lowest stone in the construction of the western ramp. The shaft descends a little over 1 E vertically, then travels horizontally outwards from the gallery, and finally drops vertically down. With the missing ramp-stone and the bridge-slab in position there would be no indication of the entrance to the well, but by lifting the bridge-slab a small opening would show in the side wall of the Horizontal Passage. From the state of the floor at the beginning of the Horizontal Passage, and from the step down of 1 E from it to the inner end of the passage-floor, it is clear that the Horizontal Passage floor is unfinished, and would have had the greater depth throughout its whole length when completed. In this state the floor would have been on the same level exactly as the floor of the short horizontal section of the well-entrance and there would have been an opening nearly 2 E high and 2 E wide in the side of the Horizontal Passage beneath the bridge-slab; so that a person in the Grand Gallery had no need to remove the ramp-stone to gain access to the well, but merely to lift the bridge-slab.



## PYRAMIDS AND THEIR PURPOSE

### ANTE-CHAMBER

This consists of a horizontal passage from the upper end of the Grand Gallery at the level of the top of the Step, to the King's Chamber, which passes through the space for housing the portcullis. The passage itself is slightly smaller than the others in the passage system, and is of limestone construction from the gallery to the commencement of the portcullis recess, but of granite thereafter.

The portcullis recess is of granite on all sides except that of the entrance from the gallery, which is limestone. It is just over 7 ft high and 6 long, and contains the slide grooves for three portcullis slabs to descend vertically on to the floor; the face of the south wall is also grooved for the passage of the innermost portcullis slab. The slides are empty, and have clearly never been used. Immediately outside the outer portcullis grooves is the 'granite leaf', which consists of two superimposed granite slabs in a groove, the bottom of the lower slab being exactly level with the roof of the entrance passage. That this was not a portcullis slab is clear from the fact that it has no grooves to slide downward from its present position; from its different thickness from the actual portcullis slides; and from its two-piece construction which would have been a joy to plunderers.

When the portcullis slabs were dropped there would have been a space left vacant above them equal in height to their descent (the height of the continuing passage); it would have provided a means for robbers to gain access to the tops of all three slabs, where their destruction would have been greatly facilitated. It is possible that the 'granite leaf', either by being pushed up (there is a boss on the upper section which would have made this easier), or by having a third section added at the top, would have served the purpose of concealing the vacant space over the portcullis and effectively sealing the entrance on all sides with granite.

### KING'S CHAMBER

This burial chamber, entirely of granite, has five superimposed roofs, each consisting of a number of massive granite beams bridging the side walls of the chamber and free from the end walls. Above the upper layer of beams is an angular vault roof of large slabs. This construction, adopted no doubt with the idea of rigidity under heavy load, creates five low spaces between the various roofs, which were named by Howard Vyse in 1837 when he had tunnelled up from the

# LOWER END OF GRAND GALLERY

To illustrate launching of plug blocks

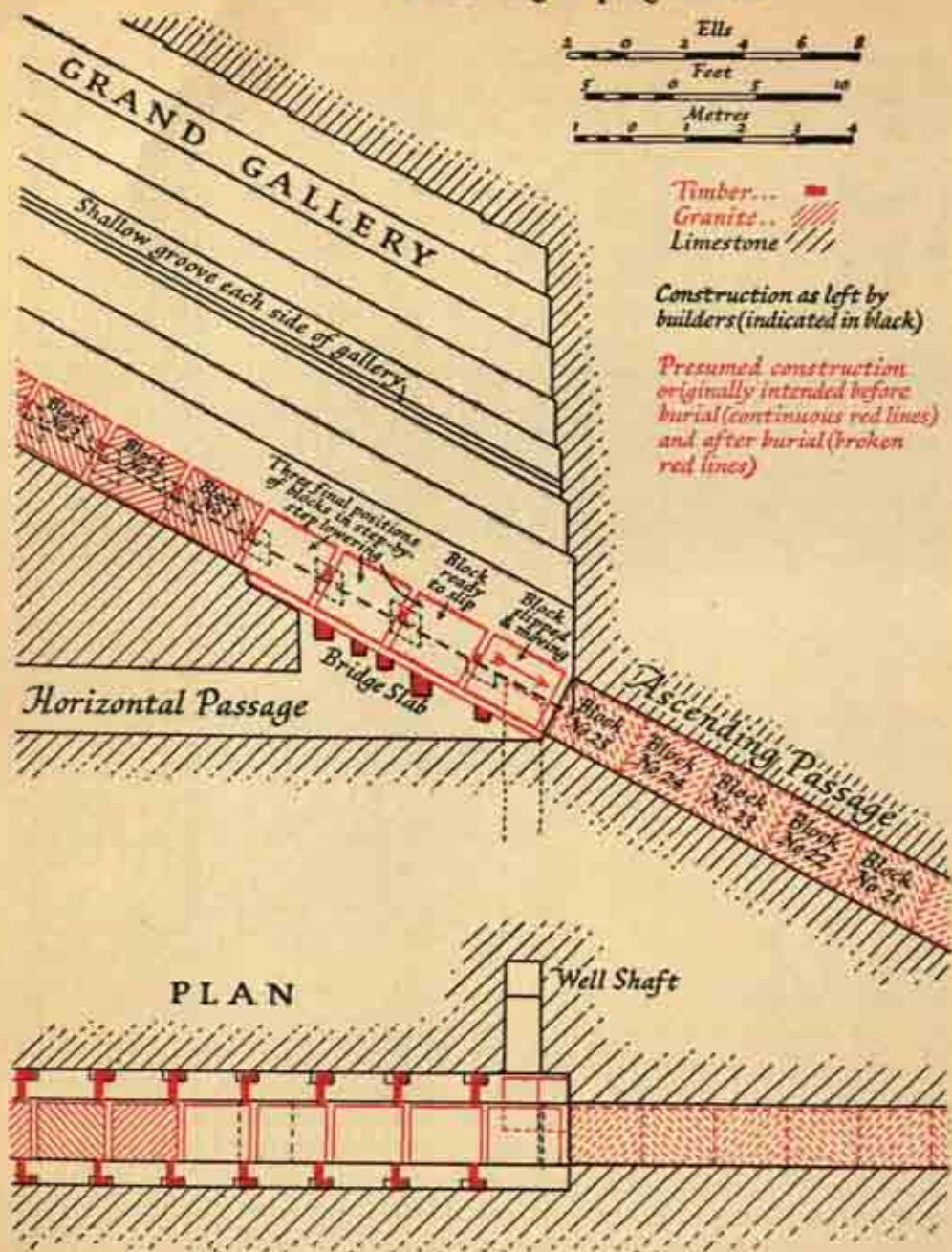


FIG. 3



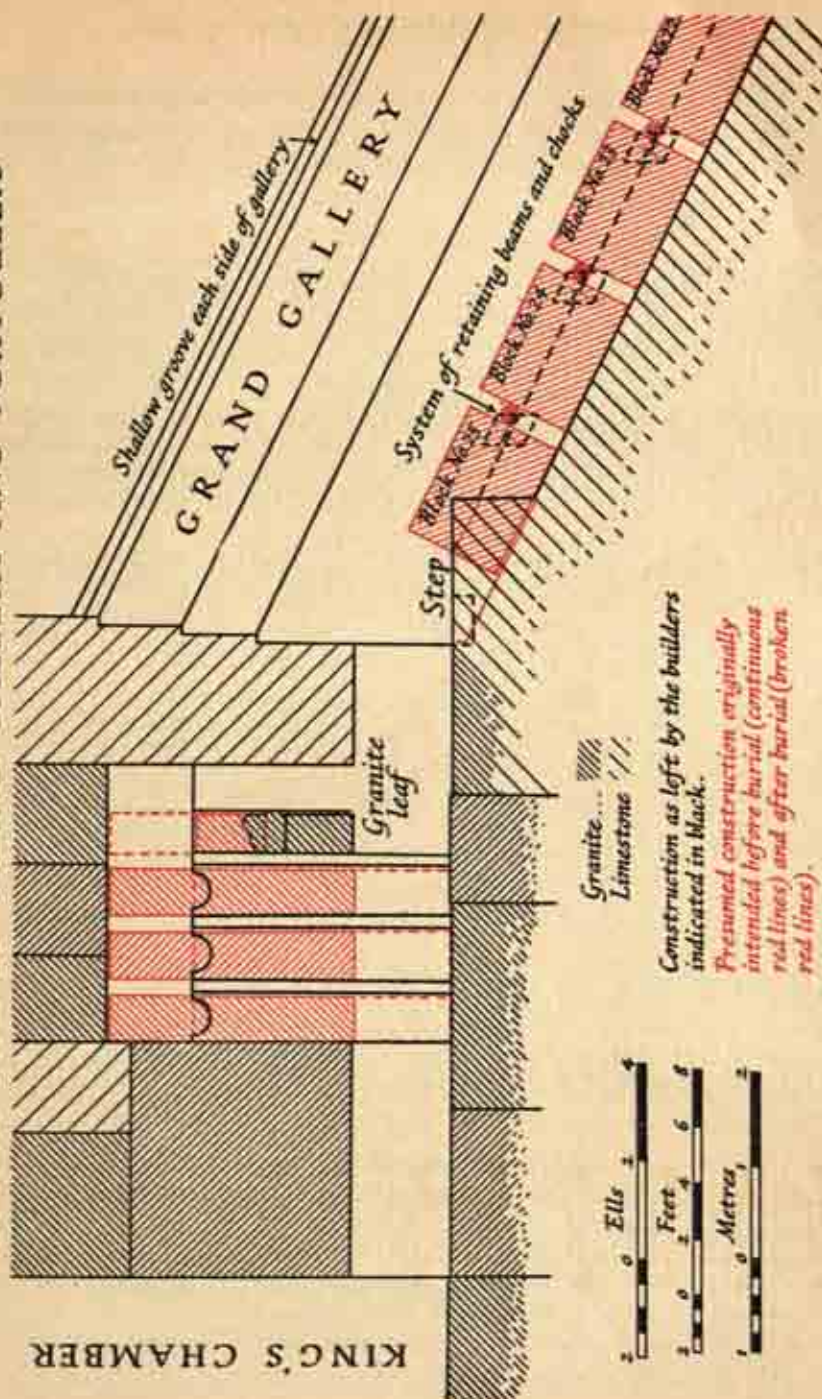


Fig. 4

## ANTIQUITY

lowest to the upper chambers. The lowest chamber, 'Davison's Chamber', was found by Davison in 1765 when he discovered the break-through to it from the upper corner of the Grand Gallery: there is no evidence to tell us when this break-through was made. These Chambers of Construction are among the few places where the unworked backs of masonry are visible, always an interesting find since the red paint marks put on the blocks in the quarries are likely to be still on the stone, as they are in this case. It is common to find in these inscriptions the king's name, the date, and the name of the gang who cut the stone, since the Egyptian quarrying was organized on a very efficient system of checking throughout each block's journeyings, to the time of its resting in its final place in the structure. Here, in the Chambers of Construction, we have the name of Khufu and the names of a number of gangs.

There is an unfinished sarcophagus in the King's Chamber—broken, lidless, uninscribed—and it was so found by Caliph Al Mamoun in A.D. 820 when he made the first break through the masonry of the pyramid since its original closing. The fact of being without inscription is not in itself strange, since this fashion was in use at the time. The alabaster sarcophagus of the king's mother had no mark on its surfaces, though those of his sons and daughters followed both fashions—inscription and lack of it. The present writer is not aware of the bottom of the pyramid sarcophagus having been examined; but in the case of the king's mother there was a sign twice repeated in sunk relief, in some way significant to the men who made the sarcophagus, cut in the underside—a chisel (identical with an actual chisel in copper found in her tomb).

The remaining feature of the King's Chamber is the existence of two ventilating shafts, one low in each side-wall, which ascend through the pyramid masonry at a steep angle and emerge through the face of the pyramid about 190 E up the face from the platform level.

### HORIZONTAL PASSAGE

This passage runs from the junction of the Ascending Passage with the Grand Gallery to the Queen's Chamber. Its floor is only finished at the inner end, where it is level with the floor of the chamber and its height is just over 3 E, the outer end being 1 E above the remainder and the height here about 2 E. As this latter height is that of the Ascending Passage, it may be that the greater height, which has not



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been completed throughout its length, was originally intended to admit the 'ka'-statue of the King by way of the then unroofed Grand Gallery. The niche for this statue is in the west wall of the Queen's Chamber.

### QUEEN'S CHAMBER

This chamber, 11 E long, nearly 12 E high, and exactly 10 E wide, is of granite and has a vaulted roof similar to that over the uppermost of the Chambers of Construction. It is in the east-west centre plane of the pyramid, while the King's Chamber and the rock-hewn chamber have this plane passing north of them and roughly through their entrance passages. In the west wall of the Queen's Chamber is a tall niche, 3 E wide at its base and tapering by overlapping courses to less than 1 E at the top, and in height exactly equal to the height of the chamber side-walls to the spring of the vault—nearly 9 E. This niche is empty and has been broken away at the back by plunderers hoping to find a way to somewhere else. The Queen's Chamber has a pair of unfinished ventilator shafts, like those in the King's Chamber, but they were only completed to within less than 1 H of the chamber walls. Wayman Dixon found them by sounding in 1872 and broke through the remaining thickness of the walls to open them up. The Queen's Chamber has no protection against robbers in the form of a portcullis, but it was probably presumed that the bridge-slab in the Grand Gallery floor was sufficient concealment.

### WELL-SHAFT

As described under 'Grand Gallery' this shaft descends from the lower end of the western wall of that gallery. Its course is erratic but interesting. After descending vertically for about 10 E it continues down more uncertainly and roughly to the rock-surface, which it reaches in what is now called the Grotto—a natural cave in the rock. From here it wavers a little before settling to a straight line descent at an angle of about 50 degrees below the horizontal, which course it follows to within about 20 E of the point at which it emerges into the west side-wall of the Descending Passage, and about 15 E from the lowest point of that passage. It descends to the level of the passage-floor and then enters it horizontally by a short tunnel. Suggestions as to the purpose of this well-shaft will be considered later.

There are two details which are not described above or illustrated in the plans. The first is the broken passage made by the Caliph



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Al Mamoun, who broke into the pyramid-surface in the north face, near the centre, but too low to strike the original entrance—which must then have been invisible as originally sealed up. He broke first into the Ascending Passage alongside the plug blocks, went down further and broke into the Descending Passage, and then returned and proceeded up the Ascending Passage into the inner passages and chambers. The second is the break through from the Grand Gallery to the Chambers of Construction, which goes straight through from the highest point of the side wall of the gallery into 'Davison's Chamber': ancient or modern, and with what purpose is not known.

### PYRAMID CONSTRUCTION METHODS

There have been almost as many different suggestions as to how the Pyramid of Khufu was constructed as there have been writers on the subject, but very few are worth considering. Petrie (*Ancient Egypt*, pt. 2, 1930) suggests a ramp occupying the full width of one pyramid face, and steepened as the work progressed upwards as far as about three-fourths of the pyramid height; thereafter by a zigzag ramp up one face to the top. This ramp would need to have extended a distance of at least 1000 feet outward from one face of the pyramid. On the east is Khufu's royal cemetery (see PLAN p. 188), with the tombs of his mother, his four queens, his sons and daughters; also his pyramid-temple, the work on which must have been in progress during the construction of the pyramid. On the north the edge of the rock-plateau is only about 500 feet from the pyramid. On the south is more of the Khufu cemetery, and the ground drops rapidly at a distance of about 400 feet. On the west is the great western cemetery of the fourth and later dynasties. There is no sign whatever of such a ramp as that postulated, which must have had a mass equal to or greater than that of the pyramid itself; but small construction ramps to some of the fourth dynasty mastaba tombs are still in place in the eastern cemetery or can be traced from their broken sections.

The question of the approach is easily solved since the causeway of the pyramid descended at an easy slope from the pyramid-temple on its east face to the valley-temple which lay somewhere under the modern village of Kafr-es-Saman, at the foot of the rock-plateau cliff. This causeway was made of large blocks of masonry, and its whole course is traceable either by the bed cut in the rock or by the actual masonry which remains where the causeway came up the cliff. At the



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time of the inundation the stone could be landed from barges at the valley-temple end.

When we come to consider the means of getting the huge blocks up to the height of their courses on the pyramid we have to think of the methods to which the Egyptians were accustomed, and the easiest way in this particular case. The ramp was the favourite method of getting great weights up to a height: the easiest way of applying a ramp to the pyramid was to use the pyramid-construction itself and leave a 'spiral' ramp in the core structure at an easy inclination such as used elsewhere, encircling the pyramid continuously until reaching near enough to the top. Such a ramp with a slope of 1 on 5, up which the blocks would have easily been moved with levers and rollers, would have risen to within 40 feet of the top in two revolutions round the pyramid: a ramp circling round the pyramid once would have needed to have a slope of only 1 on 3. The suitability of this method is obvious, and one can assume that the Egyptians were ingenious enough to have thought of it, and one can almost presume that this was the method used. The completion of the full height would have been the point at which they would have worked their way back down the spiral ramp, filling in to the finished face as they descended.

Concerning the passage-system, we have a number of unfinished pyramids to refer to, and one of the most definite is that of the first queen of Khufu, the northernmost of the three small pyramids east of his own. This queen's pyramid was originally begun about 100 feet further east than it now stands, and because of its possible encroachment on the area of the secret tomb of Khufu's mother, Hetep-heres, the site was shifted. At the time of this shift the masons had cut into the rock for the beginning of the descending passage to the tomb chamber, and had also cut the sloping bed for the massive stones which were to frame the entrance; so that we know this to have been the usual procedure in commencing a pyramid, and can safely assume that Khufu's own pyramid was begun in the same way. The rock-cutting work was done first and then the superstructure was built over it.

Of the method followed in the casing we have also some very clear examples in the mastaba tombs of Khufu's royal cemetery (eastern). There, from the unfinished sections, tool marks, etc., it has been possible to reconstruct the process pretty completely. When the core masonry was finished, the first course of casing was built up complete, each block being faced level beneath and on the side which was to butt against one already in position; the bedding being levelled to fit each



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stone individually. The bedding and the side faces were smeared with a very thin coat of fine plaster, less than a millimetre thick, which served as a lubricant on which the stone could be pushed up for fitting and withdrawn for further adjustment. The final fit of the side faces was helped by the use of a long copper saw, about 5 millimetres thick, which was worked down between the two stones to give a fitting face on each, after which the stone was pushed up against the neighbour and a very fine joint resulted.

The blocks arrived from the quarries *unworked* and with the name, date and gang-name of quarriers painted on one face of certain stones in red; possibly all stones were marked, since the only face on which such marks can now be found are the unworked backs.

The levelled bed of rock had been previously marked out with incised lines giving the base outline of the mastaba or pyramid, and when the first course of casing was in place the same thing was done on its levelled upper surface, the lines being set back a definite amount for the rise above the lower lines. This was the method always employed by the Egyptians to define, measure or state in writing a given slope, a rise of 1 E on a base of 5 H, etc. Often in scribing these lines two would be marked, one to the correct measure and another about 1 H outside it for the stone-dressers to measure back from in finishing off the surface; the correct line was of course covered by the outward excrescences of the undressed face of the stone of the course above, until the final facing brought the work exactly to the line.

The subsequent courses of casing were put on in the same way, each being completed before the next was begun, and the structure reached its full height with all the faces undressed. Each face was then dressed from the top downwards as a whole. There were exceptions to this in many instances of inferior workmanship, usually found in the tombs of poorer people or in generally poorer times, but the best work in the Giza eastern cemetery always followed it. It is by far the easiest method of carrying out such work, and would have been entirely suitable for the pyramid of Khufu. Petrie gives as a reason for believing the face to have been finished before erection that the planes of some of the casing blocks differ from that of the contiguous block very slightly; but there are several other causes which might account for this.

### MEASURES TO DEFEAT PLUNDERERS

From the earliest times Egyptian tombs showed great ingenuity in the attempts to make it impossible, or very risky, for plunderers to



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gain access. Usually they were unsuccessful, because the plunderers were even more ingenious, and the true 'secret' tomb with nothing to show above ground was the only method pretty certain to escape.

The system of sliding plug-blocks down an inclined passage to fill it was widespread over a long period, and there are many examples among the fourth dynasty tombs at Giza; a noteworthy example is the rock tomb of Wah-ka at Qau, where a very cunning but unsuccessful plug-system was used. The usual procedure of the plunderers was to tunnel alongside the plug-blocks when these were of harder stone than the surrounding masonry: the Caliph Al Mamoun gained entrance to the pyramid in this way.

Another favourite stand-by of the tomb builder was bluff. The tomb entrance appeared to be in one place, whereas the actual approach was elsewhere. The concealed opening into the Ascending Passage of the pyramid is a case in point—the plunderers being expected to go down the normal Descending Passage, find what they thought was the burial-chamber, find it rifled and assume that the burial priests had been before them—as they often were—and leave the rest of the pyramid alone.

The portcullis was another well-used adjunct, and examples of it are found in many places. A large number of the Giza mastaba tombs have a single portcullis at the bottom of a deep shaft in the rock. It was usual also to conceal as well as possible the outer entrance to the tomb. The entrance into the Great Pyramid consisted of a single block of stone which was indistinguishable from the others of the casing-face; and it was not discovered until after the plundering of 820.

From all this one can see that there was nothing new or unusual in the measures adopted in the Pyramid of Khufu to defeat the robber. It is only in the means of carrying out these measures in detail that the pyramid goes further than most other tombs, the care and thought given to the detail being remarkable.

### ORIGINAL PLAN OF THE PYRAMID OF KHUFU, AND PURPOSES OF ITS PARTS

There is some reason for believing that the original design of Khufu was for a pyramid of about 300 E-side (about the same size as that of his father), to contain only the normal passage descending to a burial-chamber in the rock, though this plan must have been discarded for a better one before the superstructure was begun. One indication of this is the height up the pyramid-face at which the entrance is placed.



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Some examples of the proportion of this height to the total slant-height are :—

Pyramid of Khufu, as it is	.. .. .	.116
Pyramid of Khafré, upper	.. .. .	.0824
Pyramid of Menkaure	.. .. .	.0632
Pyramid of Khufu, had its face been at the end of the plug	.. .. .	.05
Pyramid of Khufu, 10 E outside plug	.. .. .	.06

This last case would have meant a pyramid of 330 E-base and 210 E-height, multiples of 11 and 7 by 30. The pyramid as it is has the base and height multiples of 11 and 7 by 40, and the Meydum pyramid 11 and 7 by 25.

Another indication is the unfinished state of the 'burial-chamber' in the rock. This, one of the first works undertaken in the pyramid construction, would scarcely have been left as it is unless discarded. It would even have been a better means of deceiving robbers had it been a convincing burial-chamber and not an obviously unfinished one.

However this may be, it is certain that the present pyramid was built from the platform upwards as one complete design, without alteration, so that the change in plan, if any, must have been made before the platform and base lines were laid out. It seems likely that Khufu, who would have begun plans for his pyramid as soon as he came to the throne, would have taken his ideas from the pyramids of his predecessors and of his father in particular. One can imagine the architect who designed the present pyramid coming to Khufu with his plans and having them enthusiastically approved at once.

When we come to the internal passage-system we find a much more drastic and significant change in the plan, which implies a complete alteration in the purpose to be served by the Pyramid. Before going over the many clear signs of this, however, it would be as well to attempt a reconstruction of the procedure of the burial arrangements as originally intended. The coffin would be carried down the Descending Passage and up the then empty Ascending Passage—a portable ladder of some kind being used to mount from the one passage to the other. On entering the Grand Gallery the slab-bridge would be covering the horizontal passage to the Queen's Chamber, and the procession would probably proceed up the gallery on the plug-blocks which would then have been loaded into their places in the chute. There is accommodation for 25 of these blocks between the cross-beam sockets in the ramps, and 25 blocks of the same size as the three now in the Ascending Passage would have exactly filled that passage to an inch.



## PYRAMIDS AND THEIR PURPOSE

NO. 1 block at the time of the burial would have been about half an B above the upper end of the bridge-slab, and NO. 25 about halfway into the space now occupied by the Step at the top of the gallery.

The coffin would then have been taken in under the three portcullis slabs and placed in the sarcophagus. Omitting here any possible ceremonies connected with this, the lid would have been closed and the procession have returned to the pyramid entrance. The Queen's Chamber, which would have had the 'ka'-statue of the King in its niche, would have been the offering-room into which all the offerings, furniture, etc., would have been placed; whether before or after the coffin's journey up does not affect matters here. On emerging from the King's Chamber the priests would have seen the portcullis slabs dropped into place one by one, the 'granite leaf' pushed up or built up to seal the space now vacant over the portcullis, and would have emerged from the pyramid, leaving the plug procedure to be carried out by the gangs brought up into the Grand Gallery for the purpose.

Probably the concealing limestone block at the opening of the Ascending from the Descending Passage would be built into place and fixed immovably before the plug-blocks were released, but it is also possible that a temporary measure for checking the plug-blocks at their stopping position was used, and the limestone block put up afterwards. In any case the plug-blocks would have been lowered, NO. 1 first, step by step, from ramp-socket to ramp-socket, until each lay in the lowest position in the chute. Then each block would have been released when it reached this position, to slide down the Ascending Passage and bring up against the rear of the preceding block. A thin liquid plaster was used between each pair of blocks, and this was probably put on immediately before slipping each block.

Now we come to an interesting point. What about the gangs who were doing the plug-block work, when the first block went home? Were they expected to remain to die of starvation or suffocation? It is possible but by no means probable at this period. It is here suggested that the well-shaft was expressly constructed for their escape, either with or without the knowledge of the highest officials. If without, one can well imagine the representative of the workmen concerned getting hold of the right subordinate official at an early stage of the Pyramid construction and 'squaring' matters. It is possible that, before the pyramid superstructure was begun, they established themselves at the Grotto in the rock surface and tunnelled down to a point near the base of the abandoned Descending Passage, where no one



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would be likely to see too much of their operations. Then, as the courses of the pyramid rose, they were able with the connivance or help of the right man to have the upper part of their shaft left as a small gap in each course put in. The slight 'wandering' of the well-shaft may be due to their efforts to make a negotiable passage rather than a sheer vertical shaft which would have been a hindrance and danger to their projected escape: the vertical 20 feet or so at the extreme top would have been due to the local foreman having at that point realized where the lower end of the Grand Gallery was coming in the building.

With the well as an escape, the gangs had only to lift the bridge-slab, get down the well-shaft, seal the lower end with a block prepared, and emerge from the pyramid entrance when most expedient to themselves and those 'in the know'. Some time subsequent to this the sealing-stone would have been placed in the entrance and the pyramid be a finished task.

Assuming the above to have been Khufu's original intentions, we find a number of strange things in the actual pyramid. The details will be gone over and these discrepancies noted:—

**ENTRANCE.** The sealing stone must have been put in place, since Al Mamoun could not locate the true entrance.

**CONCEALING BLOCK** at foot of Ascending Passage. This was heard to fall by Al Mamoun and he found it later in the Descending Passage.

**BRIDGE-SLAB.** No trace of the actual slab is known, but it may well be that the workmen, when they lifted it for their escape, toppled it down the Ascending Passage and Al Mamoun removed it with his general tunnelling work. That three granite blocks have made their trip down implies that the bridge-slab did exist then.

**GRAND GALLERY.** There is a 'rail'-like groove along each side wall at a height of about 10 feet above the tops of the housed plug-blocks, which would well have carried a flooring of rafters for the tackle needed in handling the blocks and placing them. The sockets in the side ramps continue down past the bridge-slab recess to the end of the gallery, implying a step-by-step descent of each block to this point. There is a break through from the upper end of the gallery to the lowest of the Chambers of Construction over the King's Chamber. It was found by Davison in 1765, but whether it is Al Mamoun's work, or later or earlier cannot be said. If earlier it would have been original, and there seems no point in that. The Step at the upper end of the



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gallery is the interesting point here, since it encroaches on the area which would have been required for plug-block 25 in the uppermost socket position. It must therefore be unfinished and not completed as originally intended.

PORTCULLIS. This is clearly unfinished. There are no portcullis slabs, and the 'granite leaf' is incomplete as it is now.

KING'S CHAMBER. The sarcophagus is obviously unfinished. The worked faces are very poor, and the lid was never present, since the first to break into the pyramid found it missing then. He also found no trace whatever of burial, offerings, pottery, etc., and one can presume the chamber to have been empty but for the sarcophagus itself.

HORIZONTAL PASSAGE AND QUEEN'S CHAMBER. The floor of the former is apparently unfinished, being originally intended to be 1 ell deeper. The niche in the chamber is and has always been empty of statue. No mass of offerings or furniture was ever in the chamber, or there would certainly have been mention of Al Mamoun's having found it. The two ventilation-shafts from the sides of the chamber to the outer casing of the pyramid were only completed to within 5 inches of the inner face of the walls, and did not connect with the room until Waynman Dixon broke them through in 1872.

WELL-SHAFT. The lowest ramp-stone on the west side of the Grand Gallery has been removed and there is no record of when this was done. It could have been done during the escape of the working gangs, since the uncompleted Horizontal Passage floor made the opening under the bridge-slab rather small otherwise; it may have been done by Al Mamoun's men. No trace has been found of the actual stone. A sketch in Davidson's *The Great Pyramid* is referred to as evidence of the ramp-stone having been forced out from *below*, but this sketch and an examination of the surrounding stonework *in situ* show quite clearly that the stone was prized out from above, *i.e.*, from within the Grand Gallery. There is a better sketch in Edgar's *The Great Pyramid*, but in both sketches, as in the original, the breakage of edges and corners shows that a crow-bar or something of the kind was used on the right and left sides of the stone and above it at the back, to prize it out of place into the gallery. The greatest efforts were made at the side contiguous to the Ascending Passage opening, no doubt because the workers knew of the inclined seating of the stone which would release it best in that direction. There is not so much as a mark *underneath* to show any attempt to raise the stone from below, and in the confined space of the little narrow passage there to the opening of the well-shaft



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proper nothing but a modern 'jack' could have forced the stone upwards, and would not have caused the breaks on the adjacent stones on the gallery side. To anyone who has had experience of prizing blocks from masonry or working heavy stones through confined spaces it is obvious at a glance that the men who removed this ramp-stone were in the Grand Gallery.

In Mr Davidson's book it is claimed that the well-shaft was tunnelled vertically upwards from the foot to the Grand Gallery in later dynasties for a tour of inspection of tombs. Since the main Pyramid entrance was intact there would be no reason at all for such an inspection according to Egyptian lights, as their later inspections were to ascertain whether robbers had gained access or made attempts to do so. A vertical shaft such as the well, driven upwards, would have been an entirely un-Egyptian method. Had they possessed the detailed plan of the building, as the accurate arrival of the shaft at both ends implies, they would have certainly tunnelled up alongside the plug-blocks—especially as it would be known that only three blocks existed, or, if Al Mamoun's account means what is said of it, that only limestone blocks had to be dealt with after the first three; the intersection of the well-shaft with the Grotto on the rock-surface would then have been mere coincidence.

Taking all the above signs of lack of finish—3 plug-blocks only out of 25, Horizontal Passage floor unfinished, Queen's Chamber niche empty, Step uncut for receipt of plug-block 25, no portcullis slabs fitted, no lid to the sarcophagus, no reported trace of body, bones, offerings, pottery, etc., and uncompleted ventilating-shafts to Queen's Chamber—one is justified in presuming that a change took place in the purpose during construction. One can say, in fact, that this change must have taken place when the Grand Gallery roof was yet unplaced, when the King's Chamber also was open, and the roof-slab of the portcullis recess not yet in position. This would have been when the main mass of the Pyramid had reached about the 34th course, to allow the Grand Gallery floor to be completed high enough to accommodate three plug-blocks at least. The central part of the pyramid, including the King's Chamber, portcullis recess and remainder of Grand Gallery, would have risen at the same time above the 34th course probably to about the 50th to the 55th. It may well be no more than a coincidence, but there is one of those changes of course-depth, which occur many times in the height of the pyramid, at the 35th course; and it is the most noticeable change in the whole series. There is an interesting article



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on these course-depth changes, and an explanation, in Petrie's *Ancient Egypt*.

Until fairly recently there was no outside evidence of the existence of circumstances which might have caused such a change of plan; but from the information obtained from the excavation and recording of the tomb of Khufu's mother, Hetep-heres, certain conclusions have been reached which do supply an adequate reason. A sufficiently full account of the work on this tomb will be found in the Bulletin of the Boston Museum of Fine Arts.<sup>1</sup>

To sum up the facts which emerged:—Hetep-heres was the daughter of King Huni of the third dynasty, wife of Seneferu, and mother of Khufu. She died in the reign of Khufu and was buried by him near the pyramid of her husband at Dahshur. Plunderers got at this tomb and their depredations were discovered too late, in that they had opened the sarcophagus and removed the body with the jewellery which was on it. The fact—of the robbery but probably not of the missing body—was conveyed to Khufu, and by his orders a secret tomb was commenced at once and finished roughly at great speed, situated in the most important point in his own royal cemetery at Giza. The tomb consisted of a vertical shaft, 100 feet deep in the rock, leading to a rock-hewn burial-chamber, and was situated alongside the causeway of Khufu's pyramid, nearer to it even than the pyramid of the first queen of Khufu, and with the whole vast cemetery of his queens and children behind it. The shaft was filled solid with courses of fine limestone masonry in plaster of Paris, and the top course was of irregular pieces of the local nummulitic limestone, so that its secret was kept safely from the day of the re-burial until A.D. 1925.

One can imagine the feelings, and to a certain extent the actions, of Khufu on hearing of his mother's tomb at Dahshur being attacked. One can imagine also that the responsible officials at Dahshur were in a very uncomfortable position, but that they affirmed strongly to the King that no real damage had been done, and kept silent as to the contents of the alabaster sarcophagus which was re-buried.

The idea of a secret tomb for the burial and a normal tomb with superstructure for the public to see was not a new one—Seneferu most probably had done the same thing, as others did after him—and if we put ourselves in Khufu's place I think we should cast quizzical eyes at our own pyramid from that day. There would certainly be ample

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<sup>1</sup> Supplement to vol. xxv (May 1927), and xxvi, p. 76.



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reason for setting to and altering the whole intention of the pyramid ; to continue the work so that outward seeming should not be changed while saving unnecessary work by omitting what was no longer essential in the construction, and devising some entirely unsuspected site for the actual burial. Possibly, and probably, the number of persons aware of the reason for the change in plan would be very few—the King himself, and his most trusted officials only. It would have been quite in keeping for him to have arranged for a public ceremony at his funeral and a dummy burial in the pyramid, while those entrusted with the task buried him as previously planned where none other saw it.

This leaves us with the obvious question—where was Khufu buried?—the question remaining to the many who have arrived by one route or another at the conclusion that he was not buried in his pyramid.

No tomb has yet been found at Giza which could have been his. At one time the idea was circulated (it is in Edgar's *Great Pyramid*) that the tomb near to the Sphinx, called 'Campbell's Tomb', had been that of Khufu ; but this tomb is now known to be of a very much later date. Taking the procedure adopted with Hetep-heres, one can assume that Khufu would probably have chosen a position within the boundary of his royal cemetery, which limits are clearly defined, but this area has been almost entirely excavated down to the rock. The possibilities lie in the 'almost'. There is, for instance, a pre-Khufu quarry alongside and beneath his causeway, which has been filled in with large blocks of limestone and gypsum. This was done presumably to give a level floor for Khufu's causeway, which crosses it, and the filled quarry lies within a few yards of the tomb of Hetep-heres. The clearing of this would be a difficult, laborious, and expensive task ; but it cannot be said that there is nothing there but a filled quarry until the quarry has been emptied.

The area of the pyramid-temple has also remained unexcavated so far, though there appears to be little left beyond the basalt floor, across a part of which thousands of tourists trek yearly to the Sphinx, and perhaps the lowest courses or other traces of some of the walls. There seem to be no other possibilities so far as we know at present. That there was a problem connected with Khufu's place of burial was known in later Egyptian times, when the fact that the Great Pyramid was his work must have still been well known ; and the question was then put into writing as to who knew the places of burial of Im-hetep, Seneferu and Khufu, as though it were an oft-repeated query.



# PYRAMIDS AND THEIR PURPOSE

## NOTE I

TABLE OF VALUES OF THE ELL AS DEDUCED BY VARIOUS AUTHORITIES  
AT THE SITES MENTIONED

inches	
20.45	Borchardt. Nile gauge at Kuboschia. Late date.
20.47	Erman and Grapow. Berlin Dictionary.
20.58	Borchardt. Great Pyramid. (4th dynasty).
20.59	Average of 67 examples found by the present writer in the 4th dynasty tomb at Giza numbered G.1225B by the Harvard-Boston Expedition.
20.60	Borchardt. Great Pyramid burial chamber. (4th dynasty).
20.63	Petrie. Great Pyramid burial chamber. (4th dynasty).
	Davidson. 'The Great Pyramid: its Divine Message'.
	Junker. Various places in Giza west cemetery. (4th dynasty).
20.61	Petrie. Great Pyramid. (4th dynasty).
20.662	" Meydum Pyramid. (4th dynasty).
20.67	Borchardt. Ne-user-ré. (5th dynasty).
20.72	Reisner. Average of 3 examples in 'Mycerinus'. (4th dynasty).
20.78	Newton, according to Borchardt.
20.77	Petrie. Pyramid of Menkauré. (4th dynasty).
20.80	Borchardt. Nile gauge, Edfu. Late date.
20.83	" " Luxor. "
20.86	" " Kuboschia. "
20.94	" " Elephantine and Esneh. Late date.
20.98	" " Taifa. Late date.
21.07	" " Taifa. "

## NOTE II

### MEASUREMENTS OF THE GREAT PYRAMID

The inch measurement is given in brackets; the first column of figures gives the ells, the second column the value of the ell to account for this to the nearest finger. The authorities for these measurements are:—Cole, *Survey of Egypt*, paper no. 39, 1925; Borchardt and Petrie in various publications; Morton Edgar, *The Great Pyramid*, 1924; Davidson, *The Great Pyramid*, 1927.

The average value of the cubit in all these measurements is 20.614 inches.

THE PYRAMID	Ells of 7-hands			Necessary value of ell, in inches
	E	H	F	
Mean of 4 base lengths (9069) .. .. .	440	0	0	20.61
Height from platform to apex (5782.2) .. .. .	280	0	0	20.65
DESCENDING PASSAGE				
Total floor length (4535) .. .. .	220	0	0	20.62
Vertical rise (1854) .. .. .	90	0	0	20.60

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DESCENDING PASSAGE—contd.	Ells of 7-hands			Necessary value of ell, in inches
	E	H	F	
Horizontal displacement (2719) .. .. .	132	0	0	20.60
Vertical rise from junction with ascending passage to entrance (495.8) .. .. .	24	0	0	20.66
Floor length of horizontal passage at foot .. .. .	17	0	0	20.63
Height of floor of original entrance above platform (325.2)	32	3	0	20.59

### ASCENDING PASSAGE

Total floor length	75	0	0	20.60
Floor length to bottom of plug-blocks (1471)	71	0	0	20.71
Total vertical rise (680.3)	33	0	0	20.62
Horizontal displacement (1391)	67½	0	0	20.61
Perpendicular height (47.36)	2	2	0	20.61
Vertical height	2	4	0	20.60
Length of plug of 3 blocks and 2 joints	8	4	2	20.62
Length of one plug-block and joint (59.43)	2	6	1	20.60

## GRAND GALLERY

Overall floor length (1884)	..	..	..	..	91	2	0	20.64
Floor length to Step (1815)	..	..	..	..	88	0	0	20.63
Intrusive floor length of Ascending Passage	..	..	..	..	1	1	2	20.61
" " " " " "	..	..	..	..	1	0	1	20.61
Horizontal displacement of total length (1689)	..	..	..	..	82	0	0	20.60
Horizontal displacement to Step (1629)	..	..	..	..	79	0	0	20.62
Vertical rise in floor to Step bottom (840½)	..	..	..	..	40	6	0	20.57
Vertical rise to top of Step (876½)	..	..	..	..	42½	0	0	20.62
Vertical height (339.25)	..	..	..	..	16	3	0	20.65
Perpendicular height	..	..	..	..	14	5	1	20.67
Floor width (82.5)	..	..	..	..	4	0	0	20.64
Width of plug-block chute (41.98)	..	..	..	..	2	0	1	20.61
Depth of plug-block chute (perpendicular)	..	..	..	..	1	0	½	20.61
Length of bridge-slab recess (244.6)	..	..	..	..	11	6	0	20.62
Length of upper seating for bridge-slab (40.1)	..	..	..	..	1	6	3	20.61
Length of plug-block chute, bridge to Step (1546.9)	..	..	..	..	75	0	2	20.61
Length of 25 plug-blocks (1486)	..	..	..	..	72	0	3	20.61
Necessary projection of block 25 into area occupied by Step (38)	..	..	..	..	1	6	0	20.61
Maximum thickness allowable for inter-block retaining beams (8.57)	..	..	..	..	0	3	½	20.61
Intervals between similar points of beam sockets in sides of chute, each to house one block and one beam (68)	..	..	..	..	3	2	1½	20.71
Length of 1 plug-block (59.43)	..	..	..	..	2	6	1	20.60
Width of plug-block (41.8)	..	..	..	..	2	0	1	20.61
Height of plug-block (47.2)	..	..	..	..	2	2	0	20.61
Height of Step face (36.01)	..	..	..	..	1	5	1	20.50



# PYRAMIDS AND THEIR PURPOSE

							Ells of 7-hands	Necessary value of ell, in inches
GRAND GALLERY—contd.							E H P	
Face of Step from end wall of gallery (61.01)	..	..	..	..	..	..	2 6 3	20.58
Step above platform level of pyramid	..	..	..	..	..	..	82 0 0	20.63
ANTE-CHAMBER								
Portcullis recess, length	..	..	..	..	..	..	5 5 0	20.59
" " height	..	..	..	..	..	..	7 1 0	20.59
Outer passage length (52.04)	..	..	..	..	..	..	2 3 3	20.59
Inner passage length	..	..	..	..	..	..	4 6 0	20.59
Height of portcullis sections and slides (103)	..	..	..	..	..	..	5 0 0	20.60
Height of outer passage (43.71)	..	..	..	..	..	..	2 0 3½	20.61
Thickness of portcullis units	..	..	..	..	..	..	1 0 0	20.59
KING'S CHAMBER								
Length (412.5)	..	..	..	..	..	..	20 0 0	20.63
Breadth (206.3)	..	..	..	..	..	..	10 0 0	20.63
Height (230.5)	..	..	..	..	..	..	11 0 0	20.67
Height of each course of masonry (47.1)	..	..	..	..	..	..	2 2 0	20.60
SARCOPHAGUS								
External length (89.71-90.01)	..	..	..	..	..	..	4 2 2	20.59
" breadth (38.65-38.72)	..	..	..	..	..	..	1 6 ½	20.59
" depth (41.16-41.27)	..	..	..	..	..	..	2 0 0	20.61
Internal length (77.93)	..	..	..	..	..	..	3 5 2	20.59
" breadth (26.79)	..	..	..	..	..	..	1 2 ½	20.56
" depth (34.34)	..	..	..	..	..	..	1 4 3	20.61
HORIZONTAL PASSAGE TO QUEEN'S CHAMBER								
Length (1521)	..	..	..	..	..	..	73 6 0	20.59
Height (46.4)	..	..	..	..	..	..	2 1 3	20.62
QUEEN'S CHAMBER								
Height of side walls	..	..	..	..	..	..	8 6 1	20.59
Height of 'ka'-statue niche	..	..	..	..	..	..	8 6 1	20.59
Height of vaulted roof	..	..	..	..	..	..	11 5 2	20.59
Width of 'ka'-statue niche at base	..	..	..	..	..	..	3 0 0	20.59
Width of chamber (205.8)	..	..	..	..	..	..	10 0 0	20.58
Average value of Ell								20.614

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### EXPLANATION OF PLAN OF TOMBS

(see page 188)

**A ROYAL CEMETERY OF KHUFU** in which are the pyramids of three of his queens, the mastaba tombs of two further queens, his sons, daughters, grandchildren, and probably later descendants among the later tombs. The secret tomb of his mother is also here. The cemetery is bounded by the Pyramid of Khufu, his causeway, and the edge of the pyramid plateau on the west, north and east respectively, and has been in course of excavation since 1924 by the Harvard-Boston Expedition (about 117,000 sq. metres are surveyed).

**B GREAT WESTERN CEMETERY** of mastaba tombs of the 4th dynasty, which has an area of about 255,000 sq. metres. About one-third of this was excavated completely by Professor Junker's Austrian Expedition and the remaining two-thirds have been in the concession of the Harvard-Boston Expedition since before the war. The important part of this has been excavated but not published, except for the publication of the Austrian work; the unexcavated part is not of great importance.

**C CEMETERY** south of Khufu's Pyramid, consisting of mastaba tombs of the 4th dynasty with later tombs among them. Excavated by the Austrian Expedition.

**D CEMETERIES** south of Khafre's causeway in process of excavation by Professor Selim Hassan of the Egyptian University.

**E CLIFF EDGE** of the limestone plateau.

**F CEREMONIAL CAUSEWAYS** of the three pyramids. Each one led from the upper temple against the east face of the pyramid to the lower or 'valley' temple.

Of Khufu's causeway only the level track in the rock remains except at the point where it goes over the plateau-edge, where the masonry blocks remain. Where it crosses the area of the quarry the floor is of large limestone blocks with which the quarry is filled. The lower end of the causeway is lost beneath the modern village of Kafr-es-Seman, which comes up to the foot of the cliff. The upper end has not yet been excavated and planned. The subway (v on plan) is contemporary and must be about the earliest known subway-crossing of a street. Its existence suggests that the causeway was walled and not open to the general public and that the traffic of priests and interested persons into the cemetery was by the subway. No tombs exist to the north of the causeway and this area was therefore probably not part of the royal cemetery.

**AQ ANCIENT QUARRIES.** The quarry cutting around the west and north sides of Khafre's pyramid was primarily to level a platform for the pyramid building. The Menkaure quarry has a number of contemporary tombs cut in the rock faces.

**T TEMPLES.** The valley-temple of Khafre is the so-called 'Temple of the Sphinx' and is of granite. The temple alongside is in process of excavation by the Egyptian Government. The valley-temple of Menkaure is of mud-brick and here were found the slate pair of triad statues of the king (see *ANTIQUITY*, March 1935, p. 9 and pl. vi). The famous diorite statue of Khafre was found in the 'Sphinx Temple'.



## PYRAMIDS AND THEIR PURPOSE

The upper temple of Khufu has not been excavated or planned. The basalt pavement is passed over by thousands of tourists every year, most of whom do not notice it.

The queens' temples are completely ruined, only the foundation cutting of the first, a few stones of the lowest course of the second, and about six feet high of Henut-sen's remaining. Henut-sen's temple was extended in later dynasties and seems to have been in continuous use down to Ptolemaic times, when it had become the temple of 'Isis of the Pyramid'.

The temple of Khafré has been excavated, and that of Menkauré has recently been published (Reisner, *Mycerinus*).

M TOMB OF HETEP-HERES I, the mother of Khufu. Queen of Seneferu and probably daughter of King Huni of the 3rd dynasty. The only 'secret' tomb so far found in the cemetery.

QA FIRST QUEEN OF KHUFU, name uncertain.

QB SECOND QUEEN OF KHUFU, name uncertain.

QC HENUT-SEN, third queen of Khufu.

QD NEFERT-KAU, fourth queen of Khufu. Her son was Nefer-maat. Her grandson, Seneferu-khaef, compounded his name with that of his great-grandfather Seneferu, and not with that of Khufu.

QE MERYT-ITES, wife of Seneferu and Khufu.

SA KA-WAB, eldest son of Khufu, never came to the throne. He married the eldest daughter of Khufu, Hetep-heres II, who survived him; their daughter was Meres-ankh III.

SB HOR-DEDEF, son of Khufu. From his tomb an enemy had tried to erase all trace of his name—unsuccessfully.

SC KHNUM-BAEF, son of Khufu.

SD KHUFU-KHAEF I, son of Khufu.

SE Unknown.

SF Unknown.

SG MIN-KHAEF, son of Khufu.

SH SEKHEM-ANKH, son of Khufu who married Ka-aper (not royal).

SI MIN-DEDEF, son of Khufu who married Khufu-ankh (not royal).

D MERES-ANKH II, daughter of Khufu.

GA SENEFERU-KHAEF, grandson of Khufu. Son of Nefer-maat and grandson of Nefert-kau.

GB KHUFU-KHAEF II, son or grandson of Khufu-khaef I, and therefore grandson or great-grandson of Khufu.

GC MERES-ANKH III, a granddaughter of Khufu. Her mother was Hetep-heres II and her father Ka-wab.

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H ANKH-HAEF, who married Hetep-heres II. She was not buried with him or with Ka-wab.

J KHUFU-ANKH, a 'royal acquaintance' who married Min-dedef.

K DWA-EN-HOR, a king's son.

L KA-EM-NEFERT.

N KHAFRÉ-ANKH ('Tomb of Numbers').

P PTAH-SEKHEM-ANKH.

W ANCIENT BOUNDARY WALLS OF CEMETERIES.

MC MOSLEM CEMETERY.

S THE SPHINX, portrait-head of Khafré on lion's body.

X AREA EXCAVATED by the Egyptian Government over 10 years ago and so far has not been published. It is hoped that the records and plans exist since the area has been well 'dumped' over.

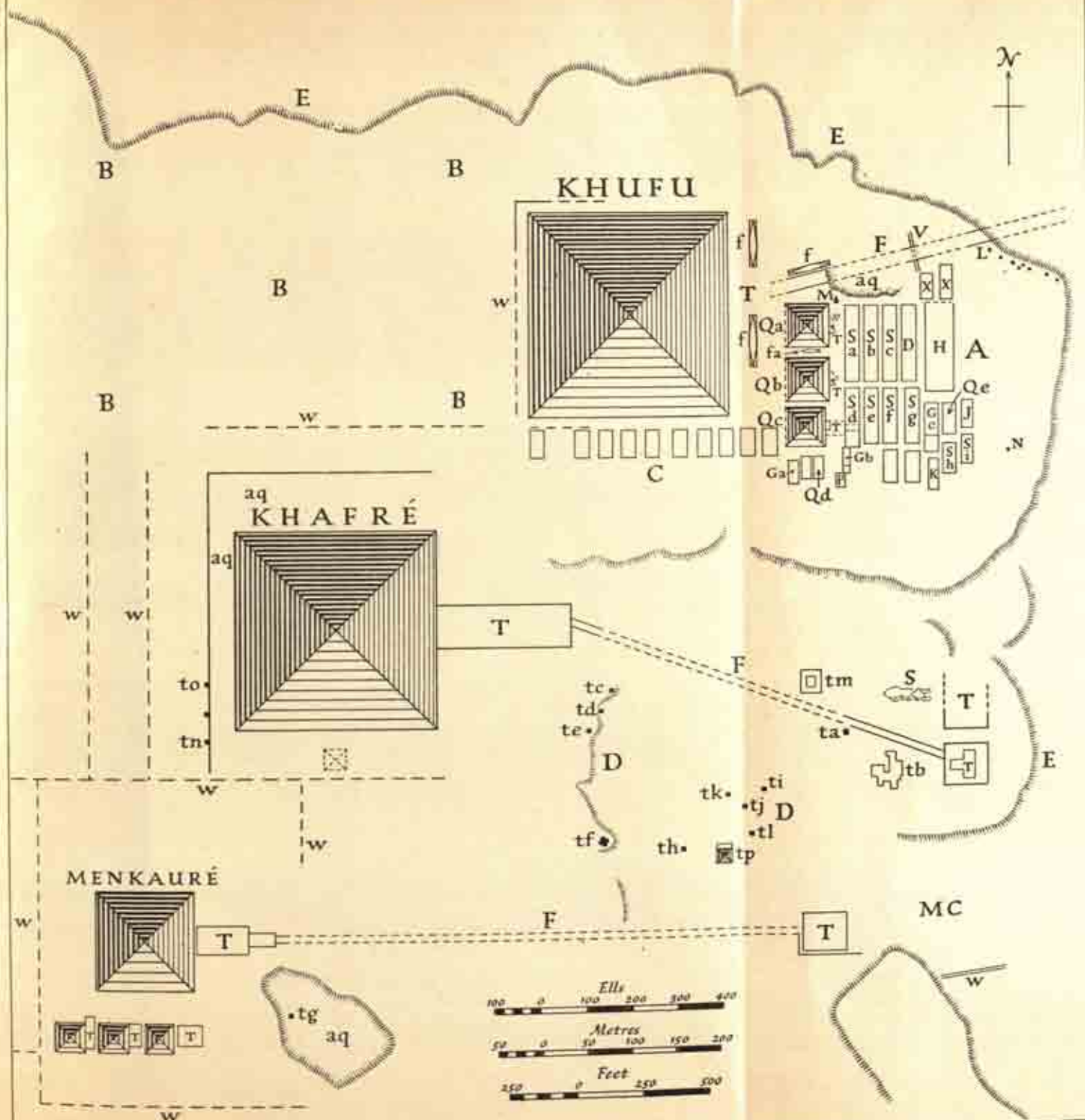
F THREE ROCK-CUT FUNERARY BOATS OF KHUFU. FA is the funerary boat of Khufu's first queen.

Assuming that we have reached the conclusion that Khufu was not buried in his Pyramid, then the probabilities point to his having placed his tomb within the area of his own royal cemetery. When we note the positions on the plan of the secret tomb of his mother, the pyramids of his queens, his ceremonial causeway, and the general 'seniority' grading of the cemetery from west to east and from north to south, the probabilities again indicate the northwest quarters of this area. The pyramid-temple, and the whole space between the pyramid and the queens' pyramids, are presumably held up by the fact that the main tourist approach to the Sphinx is over them, but eventually they will surely be cleared and the remains of the temple planned. The quarry over the block-filling of which the causeway runs, would present a costly problem if it were to be cleared, but it cannot be definitely said that there is nothing but a quarry until it is laid bare. It must be remembered that the secret tomb of Khufu's mother Hetep-heres was only found by chance in ground already cleared; the scraping of a camera-tripod leg over the plaster-of-paris between the stones filling her pit-stairway revealed the whiteness of the plaster. But for this one error of judgment of the tomb-builders, in failing to disguise the plaster as they had disguised the masonry filling itself, it is unlikely that the tomb would have ever been found. The pit filling was of fine white limestone blocks laid in courses, but the surface course was of irregular pieces of local rock. Such care in concealment by Khufu of his mother's tomb would suggest that his own tomb will scarcely jump to the eye.

If the final clearance of the whole of the royal cemetery-area should reveal nothing, then we can frankly admit without shame that Khufu has been too clever for us, and wait for chance of new evidence to disclose the place of his burial.



# PYRAMID PLATEAU, GIZA.







## PYRAMIDS AND THEIR PURPOSE

Other tombs marked on the plan.

TA RA-WER.

TB KHA-MERER-NEBTY, queen of Khafré.

TC NEB-EM-AKHET, son of Menkauré.

TD NE-KAU-RÉ, son of Khafré.

TE SEKHEM-KA-RÉ, son of Khafré.

TF MIN-YUN, son of Khafré.

TG SEBHU.

TH RA-WER.

TI WER-KHUU.

TJ KA-MENY.

TK HARSIESI (SAITE).

TL NEFER.

TM PA-KAF (SAITE) 'Campbell's Tomb'.

TN SENEZEM-IB.

TO NEB-EM-AKHET, son of Menkauré.

TP KHENTET-KAU-ES, Queen: the so-called 'Fourth Pyramid'.

## Mathematics in Antiquity

by LANCELOT HOG BEN

IN a stimulating lecture recently published in *ANTIQUITY* (1933, VII, 410-18) Gordon Childe raises the question: is Prehistory practical? He suggests that the disastrous social consequences of applying ethnological hypotheses based on flimsy foundations of fact are sufficient justification for insisting on the scientific study of prehistory and archaeology as an essential part of the intellectual equipment of a civilized person in our generation. Current events certainly sustain the justice of his plea. Still it may be argued that there is an even stronger reason, and one which is perhaps more durable, for asserting the claims of such studies to occupy a pivotal position in twentieth-century culture. The publication of Neugebauer's *Vorlesungen ueber Geschichte der antiken mathematischen Wissenschaften*\* is a timely reminder of the contribution which students of prehistory and archaeology working together can make to the solution of one of the great intellectual issues of our own time.

Modelled on the Cartesian compromise, the culture of Protestant democracy now seems to be approaching eclipse. It is the inevitable fate of a culture in which the man of science pursues his course indifferent to the gargantuan consequences of his own activities, and the man of letters has no prescience of a technological framework that has no precise historical precedent. Behind this fundamental divergence lurks another which threatens the robust materialism of Darwin's generation with an outbreak of mystery-mongering portentous of cultural decay. More and more in all its branches science relies on the use of mathematical symbolism. More and more the mathematician, divorced from experimental study, assumes the prerogatives of the calendar priestcraft in which mathematics had its origin. The reason is not far to seek. The teaching of mathematics makes little appeal to

\* Bd. 1. Vorgriechische Mathematik, pp. 212, 61 text-figures. (Bd. XLIII of *Die Grundlehren der Mathematischen Wissenschaften in Einzeldarstellungen*, edit. by R. Courant). Julius Springer, 1934. 32s.



## MATHEMATICS IN ANTIQUITY

the extroverted and socially alive. It continues to reflect the outlook of the theologians who designed the curricula of our grammar schools. In schools and universities alike those who teach it cling to Plato's doctrine that mathematics is to be studied as an aid to the spiritual refinement of the individual rather than as a social instrument for changing the real world. Since the prospect of spiritual refinement evokes little enthusiasm from healthy adolescents, they usually grow up to find the major part of modern science a closed book ; and must stomach with becoming humility whatever weak-kneed ambiguities of an archaic philosophy are rehashed for them, with a little irrelevant algebra thrown in.

To the furtherance of new methods of intellectual training in which this harmful antinomy of theory and practice in social study and in the apprehension of scientific truth need not exist, no discipline can make a more substantial contribution than prehistory. Darwinian rather than Cartesian in origin, it is humanistic in its scope and naturalistic in its method. The social historian of the written record is always liable to take the dead at their own valuation. Great as are the difficulties which arise from scanty materials, the student of prehistory is at least safeguarded against this pitfall. For him the astronomy (if any) of the men who made the Merrivale stone rows must be assessed by the orientation of their monuments ; their zoology from the animals they domesticated ; their chemistry from the beads ; the pigments and the cosmetics (if any) they used ; their physics from such knowledge of the sledge, the use of rollers or levers as they possessed. What the eldest member of the common-room said about marginal utility or the youngest undergraduate said about the dialectic do not enter the picture. Focussed on a preliterate phase in the pageant of human life, prehistory is forced to adopt a strictly behaviouristic attitude and examine a culture by its fruits. The habit is equally and lamentably rare among the critics and apologists of our own civilization.

One of the tasks with which the founders of our grammar schools were specially concerned was the policy of the open bible. Classical scholarship as an aid to textual exegesis, and incidentally to provide models of compromise between the advantages of democracy and the amenities of slavery, was a necessary ingredient of their programme. The task of our age is to make the world-view of science an open bible. A more general understanding of mathematics is an essential part of our programme. It requires little social imagination to see how mathematics, taught against its historical background of prehistorical



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timekeeping and archaeological architecture could be made the most attractive, instead of, as it now is, the most repellent subject in a curriculum of socially essential studies. Posterity may well wonder whether the real asses were those who stumbled over the Pons Asinorum, or those who graduated with honours unscathed by any suspicion of how the fifth proposition could be applied, in the ancient lore of shadow-reckoning, to construct calendar monuments of a geometrical precision challenging the architectural feats of the machine age. The Reformation democratized the art of reading. It is now time to democratize the art of calculation; and the first step is to dispel the widely prevalent superstition that mathematics was invented by leisurely and idealistic Athenians out of sheer fascination with its utter uselessness.

Neugebauer deserves our gratitude for having written the most scholarly and exhaustive account of mathematics in antiquity before the Greek period. His monograph is an authoritative examination of the textual sources and makes no attempt to trace the origins of ancient number lore and mensuration before the beginning of a written record. It is well produced and abundantly illustrated. What clearly emerges is the magnitude of the task which awaits the student of prehistory. No one would accuse Neugebauer of excessive nominalism. He adopts an essentially orthodox attitude to formalism in mathematical analysis, and repeatedly deplores the absence of mathematical good form in his sources. Still the fact remains that the Babylonians and Egyptians were able to obtain results which represent no mean order of performance. In the art of calculation Babylonian technique was far superior to that of the Attic Greeks. The great antiquity of such achievements and the close association of primitive scripts with the social activity of timekeeping suggest that we must retrace our footsteps a long way beyond the Nippur tablets and the Great Pyramid of Cheops, if we wish to understand the social origins of mathematical studies.

The reader who only wishes to familiarize himself with the main conclusions of Neugebauer's researches, in so far as they throw light on the mathematical attainments of ancient civilizations, will find the ground already traversed in Abel Rey's eminently readable and up-to-date *La Science Orientale avant les Grecs*, where some account of their social relations, especially their association with astronomy, are considered. Neugebauer has reserved the whole subject of ancient astronomy and palaeochronology for a later volume. The earlier histories of mathematics, such as those of Cantor and Rouse Ball, were compelled to base their account of Egyptian mathematics on the



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Rhind papyrus and fragmentary references attributed to Democritus and other Greek writers. The contribution of the sister civilization of Mesopotamia was still a closed book. During the past decade a wealth of new material has been added by the elucidation of the papyrus acquired by Moscow in 1912, but only made available by Struve in 1930, and by the decipherment of a large number of cuneiform texts from Mesopotamia. The larger part of Neugebauer's treatise is concerned with the interpretation of the latter.

Guided by the new material, we know that the Egyptians two thousand years before our own era were able to calculate the surface of a sphere with a precision equivalent to making  $\pi = 3.14$  and also the volume of a frustum of a pyramid on a square base. Babylonian arithmetic just fell short of being an instrument of computation as efficient as our own. The principle of position was used consistently on a sexagesimal scale, each number up to 59 corresponding to some power (positive or negative) of 60. Like the Hindu and Maya notations it employed a zero, which was intercalated to represent a gap in the sexagesimal series, as we might distinguish between 33 and 303. However, it never appears to have been used in the terminal position, as when we distinguish 33 and 330. This step seems to be all that was lacking to make the notation amenable to the Moorish devices we ourselves use. Babylonian arithmetic never actually advanced to the invention of a system of algorithms, though it might well have done so. For rapid calculation tables of multiplication, addition, subtraction, division, squares and progressions were compiled with a thoroughness in keeping with the apparatus of modern computing. The tradition of Greek arithmetic rooted in Pythagorean number-magic had far less in common with our own; and the Attic numerals, if less cumbersome than this vastly more ancient number-script, were totally incapable of giving birth to the algorithms of which every child of twelve is now the fortunate possessor. Another feature of Babylonian arithmetic is essentially modern. In expressing fractions denominators were not specified. Sexagesimal fractions were used in the same way as we use decimal fractions, except that there was no device like our dot (comma on the Continent) to signify the precise meaning to be attached to a set of figures. Babylonian arithmetical tables, like modern tables of logarithms, left the order of magnitude implied by the symbols to be inferred from the context.

The Babylonian sources, like the Chinese Tchou-Pei and Egyptian constructional geometry, bear witness to the great antiquity of the



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so-called Pythagorean relationship between the sides of a right-angled triangle. Neugebauer's cuneiform texts show a proficiency in the arithmetical solution of quadratic equations without further parallel in Greek or Alexandrian mathematics till the time of Heron and Diophantus. It is interesting to speculate upon what the later Alexandrians, like Diophantus and Theon, might have achieved in a different social context. Throughout its whole history the brilliant mathematical ingenuity of Alexandrian astronomy and geodesy was held in check by the incubus of letter symbols which could never become instruments of rapid calculation or even a satisfactory medium for the expression of large numbers. Indeed the superiority of the Babylonian notation draws attention to an issue which has received too little attention from educationists. It forces us to ask how far the much vaunted ingenuity of Platonic geometry, with its concomitant contempt for the art of calculation, was an elaborate compensation (and penalty) for the backward step taken when letters replaced hieratic numerals? Ranging through all the twelve books of Euclid, there are perhaps a dozen among about two hundred propositions necessary as a preparation for modern mathematics, and these could all be arranged in a consistent series, if the principle of proportion were accepted at the beginning. The most important demonstration in the whole of Euclid's twelve books is the thirty-first proposition in Book 1. It is also one which the average school child finds very difficult to remember. According to Rouse Ball, the Pythagorean demonstration was probably based on 'similar triangles, a method which involves the insertion of one line of construction and two lines of proof. Why did Euclid reserve all propositions about similar triangles to the last book of the geometry of plane figures? A perusal of the contents of the previous book, now generally suppressed, discloses a simple clue to the order he chose to adopt. The meaning of this long-winded and tedious catalogue of geometrical representations dealing with arithmetical generalities, now assimilated in the nursery or infant's school, is not difficult to detect, if we remember that division was a formidable and elusive operation when all calculations were performed with an abacus, when numbers were used exclusively as labels without operative significance, and when there was no way of representing the convergence of a series of diminishing fractions by the use of negative powers in a positional notation.



# Archaeology in Greenland

by DR THERKEL MATHIASSEN

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ARCHAEOLOGY in the Arctic has a charm of its own. The surroundings are unusual : the scenery is magnificent, with high, snow-clad mountains and deep fiords ; the sea is filled with icebergs or drift ice—the sun shines day and night ; seals, whales, caribou, bears, sea fowl and fish are abundant ; and the people are the small dark-haired, brown-skinned, broad-faced Eskimos, the kindest and most helpful people in the whole world.

The work is hard, for the ground is frozen a few inches below the surface ; the sun must thaw the earth, and the layers examined and removed, to expose a new frozen stratum to the rays of the sun. This frozen soil however is an advantage for everything is well preserved for centuries, as in an ice-cellar.

In my attempt to elucidate the history of the Eskimos and the archaeology of Greenland I shall give some account of work, with Eskimo excavations, for two summers in the Canadian Arctic (as a member of Knud Rasmussen's fifth Thule expedition) and seven in Greenland, a long series of adventurous and interesting years.

If the district chosen for excavation is still inhabited it is an easy matter to obtain information as to likely sites, for one has only to ask the Greenlanders. They know their country, on their hunting trips they travel all over it, and they use their eyes well, noticing everything unusual in the terrain ; and they are of course well acquainted with the remains of their forefathers. A Greenlander in Angmagssalik, on the east coast, drew for me a very accurate map of the entire district and indicated on that map more than 100 ruined villages. But that is of course exceptional. Usually one must take a Greenland pilot on a motorboat—or woman's boat—and ask him to point out the ruins of the district ; and he will do it very well. If the country is uninhabited, the problem is much more difficult. But there are many things which may serve as a guide : certain positions are preferably chosen for

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habitations—low islands and points—where conditions for building houses are favourable and where there is good hunting. The vegetation often gives hints: the grass is more luxuriant on formerly inhabited spots, where the ground has for ages been fertilized by organic refuse. Not only grass, but also flowers, often of wonderfully bright colours, may cover the old ruins and thus reveal their existence. Green spots on the coast nearly always mean an old house or camping-ground; the older the village is, however, the less luxuriant is the vegetation; and the oldest remains have the same poor vegetation as the surroundings and cannot be discovered in this way.

Many places must be visited, to find the one best suited for excavation; last summer, in Julianehaab district, I visited more than 140 old villages. Most of the places are quite small habitations, with one or two houses; most of them are dating only from the 17th or 18th century. What we require for our purpose is a site with houses which have been inhabited throughout a long period; then there will be found a large midden in front of the houses, and such a midden is the best finding-place for specimens.

When such a site is found we set up our camp, our party usually being two white people, three or four Greenlanders and a Greenland girl to do the housekeeping. The village is surveyed, mapped and photographed; the turf of some of the houses and midden areas is removed to expose the black culture-earth to the sun. Now the excavation goes slowly forward; each day one or two inches are thawed and can be excavated and removed; the specimens found are dried, prepared, labelled and packed. The uncovered stone constructions—houses, tent rings, meat caches, graves—are measured, drawn and photographed. The stone graves often contain rich finds of grave-goods.

The conditions for excavations are quite different in north and in south Greenland. In the north the ground is permanently frozen from about one foot below the surface, and thawing proceeds very slowly. The excavation takes a very long time; but on the other hand things are here usually well preserved. In the south there is no frozen ground in the summer, and even in the winter frost and thaw alternates. Excavation can proceed quickly, as in southern countries; but things are poorly preserved: all wooden and baleen implements have decayed, and often the bone specimens too, so that only a few stone objects are left as a reward for the hard work of the excavator.

Many other troubles have to be faced: in the north large frozen



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stones and whale-bones, and the water from the melting ice ; in the south willow and grass roots and often stormy and rainy weather. In Greenland the mosquitoes are an awful torment in July and August, not only the ordinary big mosquitoes, but in late summer there are millions of small, stinging flies, which get into eyes, ears, nostrils and mouth ; at many places it is impossible on calm days to work without a mosquito-net, and that is an awful nuisance. Without mosquitoes the Arctic summer would be nearly a paradise.

We will now mention a few of the places where excavations have been made, and see what results have been obtained.

The key to the archaeology of Greenland was the great find from Naujan in Repulse Bay, north of Hudson Bay in the Canadian Arctic. This village was excavated in 1922, on the fifth Thule expedition.

At the south end of a small lake, 40-60 feet above the sea, twenty ruined houses were located. The situation was such that it suggested the land had risen about 30 feet since these houses were built. The houses were half underground, round, small, about ten feet in diameter ; the walls were built of stones, turf and big whale skulls ; the roof had been supported with whale ribs and jaw bones ; the entrance to the house was through a deep, narrow, stone-set passage.

About 3000 specimens were found at Naujan, mostly from a big, three-foot thick refuse-heap, found in front of three of the houses. In the frozen earth of this midden the specimens had been very well preserved. A few blades of native copper and a bit of meteoric iron was found and most of the objects were of bone—whale bone, antler, walrus and narwhal ivory—and stone—slate, flint, jade, soapstone. Very prominent amongst the finds was baleen, shaped to all kinds of implements ; whaling had evidently played a great part in the economy of the Naujan Eskimos. Among the specimens must be mentioned harpoon heads, mostly of a special form, with open shaft socket ; arrow heads of antler with conical tang ; balls of bird bolas ; sledge shoes of whale bone and baleen ; bows of baleen ; knives with bone handles and stone blades ; drills ; adzes ; women's knives ; lamps (with a row of knobs near the front edge) and cooking pots of soapstone ; fragments of pottery vessels ; baleen cups and bowls ; many ornamental trinkets of ivory, dolls and other toys.

The culture of this Naujan find is quite different from the culture of the recent inhabitants of this region, the central Eskimos ; these people only use snow houses and are rather caribou hunters than seal hunters. When one asks these Eskimos about the old ruins they say



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that they belonged to the Tunit, a foreign people who left the country and went to the north, when they themselves reached the coast from inland. These Tunit people were small but strong; the men had bearskin trousers and the women very long boots.

This old culture in the Canadian Arctic, represented by the Naujan find, has been called the Thule culture. There is much evidence to show that this culture in a distant past originated in the neighbourhood of Bering Strait, in eastern Siberia, or in north Alaska, and spread thence to the East, across the American Arctic, where its ruins are found everywhere, both on the mainland and in the Arctic archipelago.

In the northernmost part of the west Greenland coast, in the Cape York district, is situated the trading post of Thule, Knud Rasmussen's station, erected to supply the small Polar Eskimo tribe with European goods. In 1916, close to this place, was found a very old Eskimo midden, containing many antiquities. The culture represented here is the same Thule culture found in the Canadian Arctic; we have the harpoon heads with open socket, the lamp with a wick-ledge and the many baleen objects. These finds show that the Thule Eskimos from the American Arctic also reached Greenland and were the first Eskimo inhabitants of this country.

The recent inhabitants of this region, the Polar Eskimos, have many elements in common with the Thule culture; their men have bearskin trousers and their women long boots, as the 'Tunit' of the Canadian Eskimos. And the house of the Polar Eskimos, a round stone house, is a descendant of the whale-bone house of the Thule culture.

I spent the summer of 1929 on the small island of Inugsuk, together with my young American assistant, Dr Frederica de Laguna. The island is situated ten miles north of Upernivik, the northernmost of the Danish colonies on the west coast, separated from Thule by the large, ice-filled, uninhabited Melville Bay. This small, rocky island is situated in the mouth of Upernivik's ice fiord, one of the most prolific ice-producers derived from the inland ice; the sea around Inugsuk was always filled with enormous icebergs of all sizes and shapes, dangerous neighbours, which in overturning often sent heavy flood-waves against the shore. On the west side of this small island was situated the remains of an old, big Eskimo village; most of it had already been washed away by the subsidence of the land and the action of the sea.



## ARCHAEOLOGY IN GREENLAND

A small strip of low land, about 300 feet long and 100 feet wide, was covered with a thick layer of old refuse, and had a cliff three to six feet high raised over the beach. Here bones, baleen, wooden pieces and implements were scattered. On the top of this midden were three houses, which were inhabited about 1850; but the lower and greater part of the midden was much older. The ground was frozen solid just below the surface, and it took the whole summer to excavate to the bottom; but Inugsuk proved to be the richest finding place for old Eskimo antiquities which I have met in Greenland.

Everything had been well preserved in this enormous frozen midden: bones, bone and baleen implements, heaps of big unworked baleens, wood, pieces of sealskin, feathers, eggshells, dog excrement, human hair; and the whole was saturated with blubber. On calm sunny days this old thawing midden sent forth a most unpleasant odour. It is a common rule in the Arctic that where the smell is worst, the best finds are to be got; and this was the case at Inugsuk.

About 6000 worked specimens were taken from this midden during the summer, and many of them were of great interest. Most of the implements were of types well-known from Naujan, Thule and the other sites of the Thule culture. Some of the harpoon-heads had the same open sockets; we found the same kinds of arrow-heads, bola balls, sledge shoes, knife handles, drills, adzes, women's knives, lamps (with wick ledge), cooking pots, baleen bowls, trinkets and toys. The baleen implements were very prominent: bows, sledge shoes, snow beaters, weapon points, knives, cups, large pieces of platform mats, tops, toys; even a house door, a drying rack and a drum frame of baleen. The Inugsuk people too had been great whalers.

The Inugsuk culture is not identical with the Thule culture; something new had been added, which gives the find a younger stamp: most of the harpoon-heads have a closed socket; some special west Greenland types have appeared, some implements of silicious slate, some ornamental bodkins, some antler spoons and coopered vessels. And some of these new types seem to be influenced by a foreign culture—the culture of the medieval Norsemen who in the five centuries from about 1000 to 1500 existed in south Greenland. These Norsemen were familiar with the coopering technique, and they used spoons of the same shape as those found at Inugsuk. But can these Norsemen really have had any communication with the Inugsuk Eskimos, who lived five to six hundred miles further north?

We found however at Inugsuk more evidence of this connexion:



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a lump of church-bell metal, used as a hammer ; a piece of woven cloth, of the same kind as that found in the Norse church-yards in south Greenland ; a bone chessman, converted into a top ; and two wooden carvings, made by Eskimos, but representing Norsemen ; one of them is a small doll, showing a Norseman in medieval dress, with long coat and big, loose hood, the same dress as that which has been found in the Norse church-yard of Herjolfsnes in south Greenland. The Inugsuk people must actually have seen Norsemen. Now fortunately we know that there really were three Norsemen up here in the latter part of the 13th century : for on another island, close to Inugsuk, there was found about a hundred years ago a runic inscribed stone, saying that 'Erling Sigvattsson and Bjarne Tordsson and Enride Oddsson erected this cairn on the Saturday before soccage day'. The language enables us to fix the date.

These Norse relics at Inugsuk are very important. Not only are they important for the elucidation of the connexion between Norsemen and Eskimos ; but it gives us a means of dating approximately the Inugsuk culture : it must belong to the 13th and 14th centuries. The Thule culture then must be some centuries earlier.

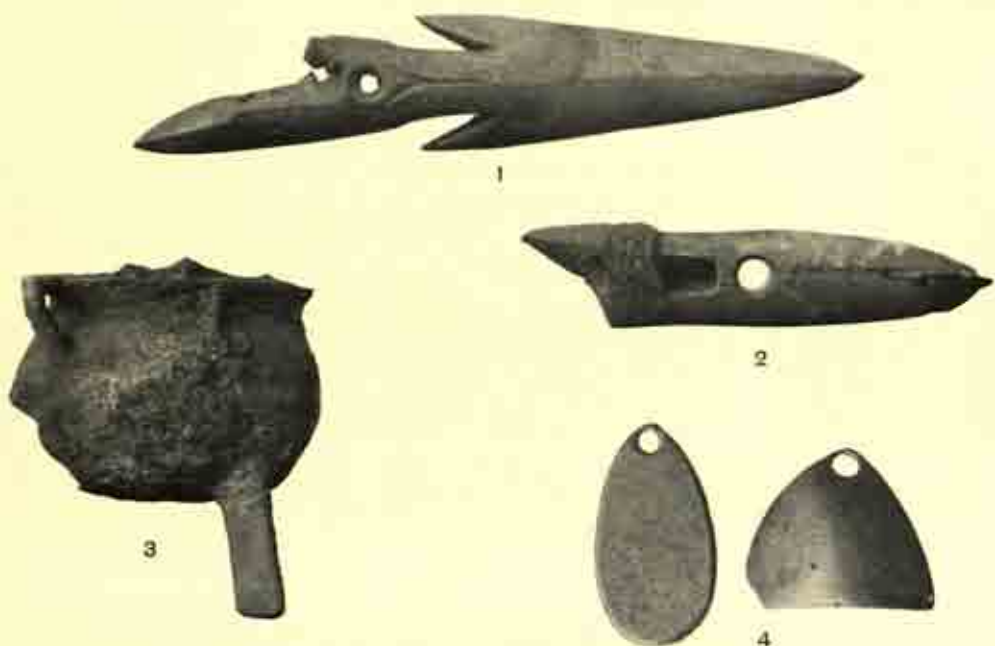
We now move into the southernmost part of west Greenland, the Julianehaab district. Here we worked during the summer of 1934, spending about five weeks at the village-site of Iugtutôq, situated at the north point of a large island. The name means 'caribou place' ; the caribou has however been extinct for a hundred years.

It was an extensive village, with ruins of 24 houses. The oldest of them are now only to be distinguished as shallow depressions in the gravel terrace, covered by heather and lichen ; others have willow bush and the latest are grass-grown.

The climate here is quite different from that we experienced at Inugsuk. It is not so cold, but the weather is more unsettled, both in summer and winter ; frost and thaw alternate even in the winter, and in summer there is much stormy and rainy weather—and any number of mosquitoes ! The excavation was easy—except for floods of melting water in the spring—but the objects were very poorly preserved ; most of the houses only contained stone objects, mostly whetting and hammer stones and soapstone pot-fragments. The houses however were well preserved : the oldest were small, round and half underground, with a deep sunken doorway, stone walls, poorly built, stone paved floor and often a kitchen, forming a bulge in the front wall, where there had been



PLATE I



5

FIGS. 1-3. HARPOON-HEADS, THULE TYPE. FIG. 3. NORSE COOKING-POT. FIG. 4. METAL EAR-DROPS.  
FIG. 5. CARVED WOOD FIGURES, REPRESENTING SHAMAN'S HELPING SPIRITS, ANGMAGSSALIK CULTURE.

# PLATE II



FIG. 6. IMPLEMENTS, ETC., INUGSUK CULTURE

1-5, harpoon-heads; 6, arrow-head; 7, pin; 8, gullhook; 9, dart with side-prongs; 10-16, stone implements; 17-19, loathins; 20-22, ornaments; 23-25, dolls; 26, comb; 27, amulet box; 28, toy lamp; 29, needle case



PLATE III

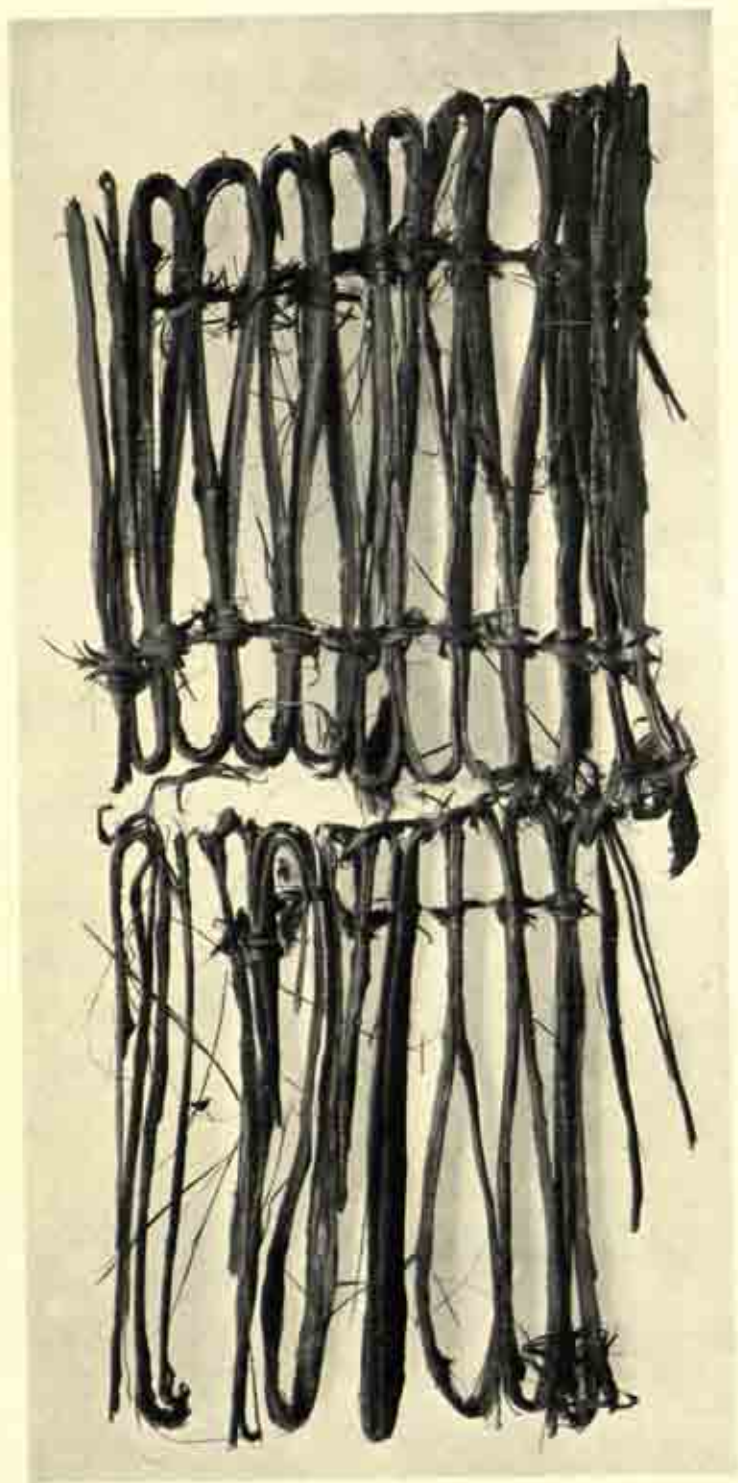


FIG. 7. PLATFORM MAT OF BALEEN, THULE.





## ARCHAEOLOGY IN GREENLAND

cooking with bone and blubber. This house-type is no doubt derived from the whale-bone house of the Thule culture; only stones and drift-wood have replaced whale bone.

The specimens found in the twenty excavated houses of this type were not very numerous; but they were sufficient to tell us that the Eskimo inhabitants belong to the Inugsuk culture. Very prominent amongst the finds were fragments of soapstone lamps with wick-ledge and objects derived from the Norsemen: pieces of bell-metal and of iron, soapstone, spindle-whorls (one with a runic inscription), a piece of woven cloth, fragments of soapstone vessels, and net-sinkers.

The Julianehaab district in the medieval period was the centre of the Norse colonies in Greenland, the 'East Settlement'. Here the Icelanders settled as sheep and cattle breeders, after Eric the Red had discovered Greenland in 985; and here for five centuries there lived a numerous population, with 190 farms, several churches and monasteries. At first the Norse Greenlanders were quite prosperous; but from the 14th century onwards communication with Iceland and Norway became less regular until it ceased entirely at the beginning of the 15th century; and when white people again went to Greenland in the 16th century the Norsemen had disappeared, having died out; only the ruins of their houses told that the land had formerly been peopled by a white race. What had happened?

The Norsemen first met the Eskimos about 1200, on hunting trips to the northwest coast; as the Inugsuk find showed, there had been some connexion between Norsemen and Eskimos. The Eskimos only inhabited the west coast as far south as the climate was Arctic, with that winter ice which was necessary for their ice hunting and dog sledges. But in the 14th century the Eskimos began to move southward. About 1350 they attacked and destroyed the Norse 'Western Settlement'; and in 1379 we hear about the first attack on the 'Eastern Settlement'. And now the Norse colonization began quickly to decline.

The Norsemen were then already a degenerate race, as Dr Poul Nörlund's excavations in the church-yard at Herjolfsnes has shown: they were sick, under-nourished, degenerate and weak. The Norsemen of course looked with contempt on the Eskimos, these small, black heathen; and they probably killed them whenever they met. But the Eskimos had a fighting method of their own: they attacked the Norsemen individually and from behind with their very effective weapons, bows and slings; or they burned the Norse houses, after



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having blocked up the doors. The tales of the Greenlanders relate how the last chief of the Norsemen, Ungortoq, fled out of his burning house with his little son in his arms, and then, when the Eskimos pursued him, threw the boy into a lake so that he should not be taken alive.

The Norse colonies disappeared and the Eskimos plundered the habitations, which explains how they got all those Norse objects which we now find in the old Eskimo ruins. The Norsemen certainly did not break their church bells voluntarily so that the Eskimos might make hammers or ear-drops out of the fragments.

Half-way up the fiords we now find the ruins of these, the oldest Eskimo villages; there they could still do some hunting from the ice in the winter, as they were accustomed to from their northern home. But later they scattered all over the country, and also onto the outer skerries; their kayak technique was now sufficiently developed to stand the south Greenland winter.

The later houses at Tugtutôq belong to this period, the 17th and 18th centuries. The houses are now big, square communal dwellings for several families, each containing 30-50 persons. The walls were of stones and turf, the floor paved with flat stones, the platform and roof of drift-wood. It was the kind of house still used when the Danish colonization of Greenland began with Hans Egede in 1721, and which is still used in Angmagssalik on the east coast. These house-ruins contain Eskimo implements of more modern type and also glass beads, clay pipes and iron goods, brought to Greenland by the Dutch Whalers.

The Eskimo immigrants to the Julianehaab district soon passed on south through the region of Cape Farewell, to the east coast. The oldest house-ruins at Angmagssalik are of exactly the same form and contain exactly the same type of implements as the oldest houses at Tugtutôq; already early in the 15th century the Eskimos had reached Angmagssalik. They did not however stop here, but wandered still farther north. The oldest houses in northeast Greenland also contain Inugsuk culture. But soon this culture was intermingled with other elements, brought by some Eskimos who from the Thule district wandered on northwards and reached northeast Greenland, where they lived for centuries, but only once did white people see them: Clavering met a small party of them on Clavering Island in 1823. Since then they have all become extinct.



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In Angmagssalik there is however still a prosperous Eskimo population ; since 1894 a Danish trading post has been situated there. A curious and in some respects old fashioned (but in others, highly-developed), culture was found amongst these Angmagssalikers, when the first white man, Gustav Holm, visited and spent a winter amongst them in 1884. In 1925 a hundred of them were transplanted to the newly founded colony on Scoresby Sound. East Greenland now has a population of about 1000 Greenlanders, while the west coast has about 15000, and the Thule district 300.

Civilization has now come to the Greenlanders. In South Greenland fishing has more and more displaced seal-hunting ; the yawl and motor-boat have succeeded the kayak and woman's boat ; the rubber-boot the kamik shin-boot ; the gun the harpoon and the wooden house the old turf hut. The old Eskimo culture in Greenland will soon exist only in the old house-ruins and graves, and in the museums.

## Prehistoric Antiquities of Malta\*

by DAVID RANDALL-MACIVER

THERE was certainly room for a well illustrated work, providing at once a conspectus and a review of early Maltese archaeology. Only an unusual library can supply simultaneously to the reader the *Papers of the British School at Rome*, three or four volumes of *Archaeologia*, the official reports on the excavations, Ebert's *Reallexikon*, and Zammit's *Prehistoric Malta*, to mention only the indispensable minimum. Even with all these the student has not enough pictorial material to enable him to visualize the whole subject.

Signor Ugolini is therefore doing a very useful thing when he undertakes to publish in six volumes of no excessive bulk an illustrated record of the monuments, sculptures and other objects of interest. He certainly possesses the first qualification for this task, as his photographs are excellent, and the line drawings and plans are up to the best standard. The author's efforts have been well seconded by the publishers, who bring out his work in handsome form with large clear print and admirable reproductions.

The volume before us is written in popular style, obviously with the hope of attracting a large public. It is not unnatural that in these circumstances the author sometimes writes to please the gallery. This is fairly harmless and does not work much damage. The book is intended at once as a preface and a general introduction to volume 1, 'The Neolithic Temples of Tarxien'; vol. 2, 'The larger Neolithic temples and the Hypogeum'; vol. 3, 'Minor Neolithic temples and Megalithic Monuments'; vol. 4, 'Temples, cemeteries and materials of the Cuprolithic age'; vol. 5, 'Conclusions'.

I have thought it convenient to translate the Italian titles, and to avoid later explanations may say at once that in this article I shall replace 'Cuprolithic' throughout by 'Chalcolithic', the word generally

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\* *Malta: origini della civiltà Mediterranea*. By Luigi M. Ugolini. Pp. xvi, 314, 12 plates and 114 text-figures. Rome: Libreria dello Stato, 1934. 100 lire.



## PREHISTORIC ANTIQUITIES OF MALTA

used by English writers. It is a better term, when 'Chalkos' is understood, as it should be, to include copper as well as bronze. Either word is preferable to the barbarous 'eneolitico' which has dominated Italian archaeology for two generations. The mulatto offspring of two classical languages, 'eneolitico' is also an execrable pun which has at last become intolerable. It is to be wished that French and German writers would now come into line by substituting 'Chalcolithic' for 'Age du cuivre' and 'Kupferzeit', which do not nearly so well express the characteristics of that phase in which the latest neolithic products coincide with the earliest appearance of copper.

The scheme of the book is, very briefly, as follows:

The first 20 pages are introductory, dealing in short summary paragraphs with the geography, geology, palaeontology, and anthropology of the island. Chapter 1 then enumerates the several types of monumental remains: caverns, tombs, temples, and introduces us to the stone sculptures, terra-cotta figures, animal-reliefs and pottery. A 'stylistic review' of these is made in chapter 2, which ends the first or expository section, written with a clear directness of style which is both practical and attractive.

In the next part chapter 3 brings up the great question on which the main interest of Maltese archaeology depends. Are the earliest temples Neolithic or not? and, if perhaps rightly classified as Neolithic on technical grounds, are they yet really retarded survivals chronologically to be equated with the Bronze Age in the other parts of the Mediterranean?

Chapters 4, 5, 6 of part 2, entitled 'Religion', 'Civilization', 'Peoples', contain some interesting speculations. The arguments in favour of considering the larger buildings to be temples rather than palaces are well mustered. Close observation and much originality of suggestion mark the account of the architecture.

Part 3 is principally devoted to an enquiry as to how much the other Mediterranean countries and islands can offer for comparison with Malta. If the results of the enquiry are largely negative yet some interesting evidence is obtained, especially from the comparison with Sardinia and with Spain.

Part 4 is far inferior to the rest of the work. It is entitled 'Conclusions', but as the real conclusions had been given already this section is mostly padding and irrelevance. Perhaps some extra pages had to be written in order to space out the numerous illustrations. But they might have been better used. Here for instance the writer



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might have found space to repair an omission which must be very patent to English readers, by giving some credit to those who have worked in Malta before him. The names of Thomas Ashby and Eric Peet do not appear anywhere at all in the text. Nor is there the slightest hint of the importance and value of Sir Themistocles Zammit's work in excavation. It may be hoped that this oversight will be rectified in the later volumes.

The questions raised in parts 2 and 3 are of vital interest to Archaeology. First and foremost is the problem of chronology, which our author tackles very courageously and, in my judgment at least, quite successfully. His argument is based principally upon a section obtained in Zammit's digging of Tarxien. From the pavement of the temple up there was a uniform layer of light-coloured earth, 90 centimetres deep, in which lay numerous objects, some certainly *in situ*, others lying confusedly. These objects included statuettes of limestone, flint implements, horns of animals, pottery models of buildings, pottery vessels. Above this was a stratum clearly to be distinguished by its black colour, carbonaceous and evidently produced by burning. This upper stratum was 30 centimetres deep and contained human bones, being in fact a small burying ground. With the human bones were objects of personal adornment, copper weapons and pottery. A photograph and a sketch illustrate the section. Now the upper stratum, our author maintains, is not as usually stated merely 'Bronze Age', it is definitely and precisely *Chalcolithic*. In this he is unquestionably right, the types of the daggers and axes prove it beyond all doubt. The lower section he maintains to be Neolithic, which is the usual view. As the buildings of the temple must be older than this lower stratum of objects the buildings, it is argued, must be Neolithic.

Some students, while not questioning the Neolithic type of the Maltese culture and the earliest temples, have maintained that they are retarded survivals, chronologically to be equated not with the Neolithic but with the Bronze Age in more progressive parts of the Mediterranean. Ugolini vigorously combats this view, which certainly seems to be arbitrary and to offer too facile an explanation. The only real attack that might be made on his position is different. Accepting the chronological equation of Malta with other countries, and discarding the suggestion of retardation, it might be argued that both strata are Chalcolithic, and that the interval between the abandonment of the temple and its filling up by the lower stratum of earth was so short that the temple itself might not antedate the Chalcolithic.



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This would be logical but it looks like forcing the evidence. The natural course for an unprejudiced critic is to accept the argument of this book and fearlessly to face the consequences.

The consequences are serious, but need not be portentous. Ugolini makes the most of them, and falls sometimes into obvious exaggerations. Yet he is certainly entitled to much credit for his fearlessness, and for the emphasis that he lays upon the importance of this orientation of the facts. Malta exhibits the earliest civilization, after the Palaeolithic, known in the Mediterranean; and it is a civilization of a surprisingly high order. Is this incredible? We have no right to say so.

The difficulty is that there is so little extant of true Neolithic age which can serve as a basis of comparison. It has frequently been remarked in late years by such writers as Antonielli, Rellini, von Duhn, that the evidence for true Neolithic in Italy is fast evaporating. The latest witness is Rellini's book on *La più antica ceramica dipinta in Italia*, which proves the painted pottery of the Vibrata and the Materano to be not Neolithic but Chalcolithic. Site after site once classed as Neolithic proves on examination to belong to the next stage.

There certainly remain in Italy and Sicily, if hardly anywhere else, a few purely Neolithic sites; but the yield from them is so slight as to afford no evidence of the general standard attained by their inhabitants. In Crete the Neolithic stratum still awaits exploration. In short there exists no criterion by which we can judge what the Neolithic civilization was really like in any part of the Mediterranean littoral or the islands in that sea.

But it is surely reasonable to believe that it was of a very high order, and I see no cause for thinking that it may not have been as high as that which is claimed for Malta. The brilliant Chalcolithic civilizations of Egypt, Crete and the nearer Orient cannot have been suddenly evolved. When we have found the preceding stage in these countries shall we be surprised if it proves to be fully as sophisticated as anything illustrated in this book?

But though I may be willing to believe in the Neolithic dating of the earliest Maltese temples, and may not be afraid to accept this interesting architecture and elaborate ornament, these characterized statues, and remarkably perfected pottery, as truly Neolithic in date and style, I cannot accept the unnecessary corollary which Ugolini draws.

For him Malta is not only the earliest seat of any civilization yet known after the Palaeolithic, but it is also the source of many features in



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the principal civilizations of the Mediterranean. This is not only implied in the title of his book, it is explicitly stated in chapter 7 and most emphatically repeated in the 'Conclusions'. It is needless to say that there is no real evidence for such a fantastic contention, but I may point out that what little we know is directly opposed to it. Nothing for instance could be more striking than the total absence of any trace of Maltese influence in Italy and Sicily, though they are geographically so near. Apulia may always have some surprises in store, but as yet Molfetta and Matera have produced nothing even faintly suggestive of Malta. In the eastern Mediterranean even Ugolini himself does not venture to suggest that Egypt borrowed from Malta. And though he claims that some pottery from the Neolithic stratum in Crete is identical with Maltese examples this scarcely helps his case. Crete was never exactly wanting in originality and may have produced its own pottery without assistance. The most that might be conceded in this regard is that Malta may possibly have preserved certain traits once widely prevalent, of which the examples in Aegean regions are still underground.

But when we turn to the western half of the Mediterranean the case is very different. This may well have been the sphere of Maltese influence; it certainly defines the circle within which that culture was evolved. There is an immediate and unmistakable link in the megalithic style of building which runs through all the islands and is found in Spain. It is true that none of these megalithic structures is known to date back to the Neolithic period, but they all have a certain generic affinity, and some of them retain in the Chalcolithic or even the Bronze Age such marked peculiarities of Maltese architecture as the semicircular exedra; striking examples of this are figured from a 'Giant's tomb' in Sardinia and from a building on the Spanish site of Los Millares.

What may have been the character of any Neolithic civilization in North Africa we cannot even guess; but I strongly sympathize with the suggestion that Tunis or Tripoli may some day help to explain the Maltese problem. In the meanwhile Ugolini's work serves as an admirable *mise-au-point*, beyond which it is impossible to go until more excavations have been made in many places besides Malta.



## Notes and News

### AN ASSYRIAN CAMP-SCENE (PLATE I, p. 216)

The Editor having asked me to write a few lines in explanation of the subject of PLATE I I gladly comply, though there can be little novelty in my remarks. This fragment of an Assyrian sculptured slab is now in the Vorderasiatische Abteilung of the National Museums in Berlin, numbered VA 965, and exhibited in the recently-opened room 15. It has been several times published and described, last by D. Opitz in the *Archiv für Orientforschung* VII, tafel III, and p. 11f. The Berlin Museum acquired it indirectly, for it was at one time in the Crystal Palace at Sydenham, but its provenance is known to be the mound of Kuyunjik (Nineveh), opposite to the modern town of Mosul on the Tigris. There it was discovered by the excavators who were working on behalf of the British Museum in 1853 and 1854 among the ruins of the North Palace, built by Ashurbanipal, the last great king of Assyria, who reigned from B.C. 668 till about 630.

In its present form the fragment preserves only part of one out of (probably) three registers which filled the complete height of the slab, and that remaining is most likely the uppermost, for the perimeter-wall of the camp, seen at the top, begins to curve downwards at the left, and the complete circuit would evidently occupy much room below the existing register. It is a reasonable assumption, therefore, that the whole slab was occupied with camp-scenes similar to those preserved, and, since the personal affairs of the king were invariably the subject of these sculptures, we might go further and suppose that the royal tent itself was shown, in the middle register now missing.

It will be seen that the moment chosen by the artist for depiction is that of the return of the army to camp after a battle, also that the quarters shown are evidently those of the high officers, which gives a further point to the suggestion above that the king's tent was pitched not far away. The returning warrior is greeted by a batman with refreshment (which he snatches greedily), another makes his bed, and in a neighbouring service-tent a sheep is being quartered for his meal. The two utensils hanging on the branch pole of the tent are not easy

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to identify—perhaps one of them is a fan to cool the master while he rests.

Outside lie sheep and a goat, also a pair of camels, or rather dromedaries, with their fodder before them, which has been brought by a man seen with his hand in a sack—though this detail has also been explained as showing a baker preparing his flat cakes by plastering them on the side of an oven. The camels neglect their meal, being occupied in courting. They have, however, an interest here beyond that of illustrating a commonplace detail of animal behaviour, for it is their presence which has caused some to believe that this scene comes from a series of pictures of Ashurbanipal's campaign against the Arabs. It is known that one room in the North Palace was lined with sculptures depicting the rout of the Arabs and the burning of their encampment, and it is assumed that this fragment belonged to the same series. But there is really nothing beyond the mere presence of the dromedaries, the Arabian camels, to suggest this, and such animals were widely used at that period in many territories over which the Assyrian kings campaigned.

There is no sufficient reason, then, for us to pitch this camp in any particular locality, though no doubt at one time the place would have been clearly indicated by an inscription upon a neighbouring slab ; but this guidance is now lost.

C. J. GADD.

## FLINT ARROW-HEADS FROM THE GRAVE OF MES-KALAM-DUG, UR\*

The complexity and richness of the civilization revealed by the excavations at Ur, and the beauty and intrinsic value of many of the objects found, are such that the flint arrow-heads from the grave of Mes-kalam-dug, illustrated by FIG. 1, may hardly appear worthy of attention. Yet it is true that in archaeology common things are normally of more fundamental interest than rare ones—'common things' wrote General Pitt-Rivers 'are of more importance than particular things, because they are more prevalent'.<sup>1</sup> This dictum of the General's must have been unpopular at the time he made it, when most collections were being formed on directly opposite lines by the assemblage of those rarities which now alternately bore or intimidate

\* Thanks are due to Dr Woolley and the British Museum authorities for permission to publish this description and for facilities in having tracings made.—EDITOR.

<sup>1</sup> General Pitt-Rivers, *Excavations in Cranborne Chase*, vol. IV, 1898, p. 27.



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the public; but at the present day we should all be agreed that the General never made a sounder or truer statement.

The arrow-heads from Ur, to which we may now draw attention, were found 'all in a cluster, point—or rather cutting edges—upwards and 0.30 m. above the floor of the pit; they had evidently been in a quiver, which had perished, and judging from their position the shafts to which they were attached had been 0.30 m. long'.<sup>2</sup> If the arrow-shafts were really only 30 cms. (*i.e.* about one foot) long they could have been of little real use. Sir John Evans had in his collection a number of complete arrows of this kind from Egypt, and these measured on the average 35 inches in length.<sup>3</sup>

The three flints illustrated belong to a class found widespread over North Africa and Western Europe. Though varying in detail, the various forms are fundamentally the same; they consist of sections of flint flakes with two sides blunted abruptly by steep flaking in the



FIG. 1. CHISEL-ENDED TRANSVERSE OR TRANCHET ARROW-HEADS OF FLINT, FROM THE GRAVE OF MES-KALAM-DUG AT UR (†)

microlithic manner; the two edges, which formed parts of the edges of the original primary flake, are left untouched, one to act as the business edge of the chisel-ended arrow, the other for insertion into the shaft. The chief possible variations in form are summarized below:—

1. The width of the edge may be several times smaller than the width of the primary flake from which it was taken (*e.g.* FIG. 2, no. 11), or may on the other hand be approximately equal (*e.g.* FIG. 2, no. 4), or as much as twice as big (*e.g.*, FIG. 2, nos. 18, 19).
2. The angle of the business edge, which is normally approximately at right-angles to a line bisecting the angle formed by the blunted sides, may occasionally be oblique (*e.g.*, FIG. 2, nos. 7, 16, 17).
3. The degree of the angle formed by a prolongation of the blunted sides, varies greatly according as these edges are almost parallel to one another or taper to a narrow tang.
4. The degree of concavity of the blunted sides, is sometimes nil (*e.g.*, FIG. 2, nos. 9, 15, 22) and in other cases quite marked (*e.g.*, FIG. 1, nos. 1-3; FIG. 2, nos. 6, 8, 18, 19).

<sup>2</sup> C. L. Woolley, *Ur Excavations*, vol. II, *The Royal Cemetery*, p. 160.

<sup>3</sup> Sir John Evans, *Ancient Stone Implements of Great Britain*, 2nd ed., p. 369.

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The specimens from the grave of Mes-kalam-dug are of closely similar form, showing sides strongly tapering and markedly concave, features which give them well-defined tangs. It is possible that this variant had become stereotyped in Mesopotamia at that date.

There is no direct evidence to suggest the exact method of hafting Mes-kalam-dug's arrow-heads, but this can be inferred fairly certainly from the evidence of finds made elsewhere in which the shaft, or part of it, has survived. Several such finds have been made in northern Europe. We illustrate (FIG. 2, no. 1) a specimen found at a depth of between  $2\frac{1}{2}$  and 3 metres in a peat bog at Tvaeremose, Eising Sogn, Ginding Herred, North Jutland,<sup>4</sup> and another (FIG. 2, no. 2) from Petersfehner Moor, Oldenburg<sup>5</sup>; in both cases the foreshaft is of wood<sup>6</sup> and the flint is secured by animal sinews. In the case of the specimen from a peat bog at Vissenberg, Odense, Fünen, engraved by Madsen and reproduced by Evans,<sup>7</sup> the shaft was also of wood, but the binding was of 'fine bast-fibre'.<sup>8</sup> Another group of specimens with hafts intact comes from Egypt. The example illustrated (FIG. 2, no. 3), belongs to the period of the Middle Empire,<sup>9</sup> and in common with other Egyptian specimens is secured in its haft by bitumen.<sup>10</sup> The shafts of the Egyptian arrows generally consisted of reed and the foreshaft of wood. The shaft was notched at one end for the string of the bow and in some cases at least there are definite indications of feathers at either side to steady the flight of the arrow (Evans, *op. cit.*, p. 369). It seems probable that the specimens from Ur were hafted in the same way.

In Northern Europe the *tranchet* arrow-head is found abundantly at sites dating from the latter half of the Mesolithic period. It is absent from the Early Tardenoisian, uncommon in the Middle Tardenoisian, but extremely abundant in the Late Tardenoisian. It is also absent, or virtually so, from sites of the axe-cultures of Northern Europe of the Boreal period, becomes fairly common in the Oldesloe

<sup>4</sup> Sophus Müller, 'Archaeologisk Udbytte af Mosearbeidet i Krigsaaret 1917', *Aarbøger*, 1917, p. 149, fig. 1.

<sup>5</sup> Gustav Kossinna, *Die Indogermanen*, 1921, p. 29, fig. 40.

<sup>6</sup> As the total length of arrow surviving is only  $4\frac{1}{2}$  inches, it is not possible to say whether the shaft was a simple wooden one, or whether it consisted of shaft and foreshaft.

<sup>7</sup> Evans, *op. cit.*, p. 409, fig. 344.

<sup>8</sup> Another Danish specimen is in the British Museum. See *Stone Age Guide*, 3rd ed., fig. 181. Material of binding not indicated.

<sup>9</sup> J. de Morgan, *Prehistoric Man*, fig. 41, no. 6 and p. 97. <sup>10</sup> See Evans, p. 369.



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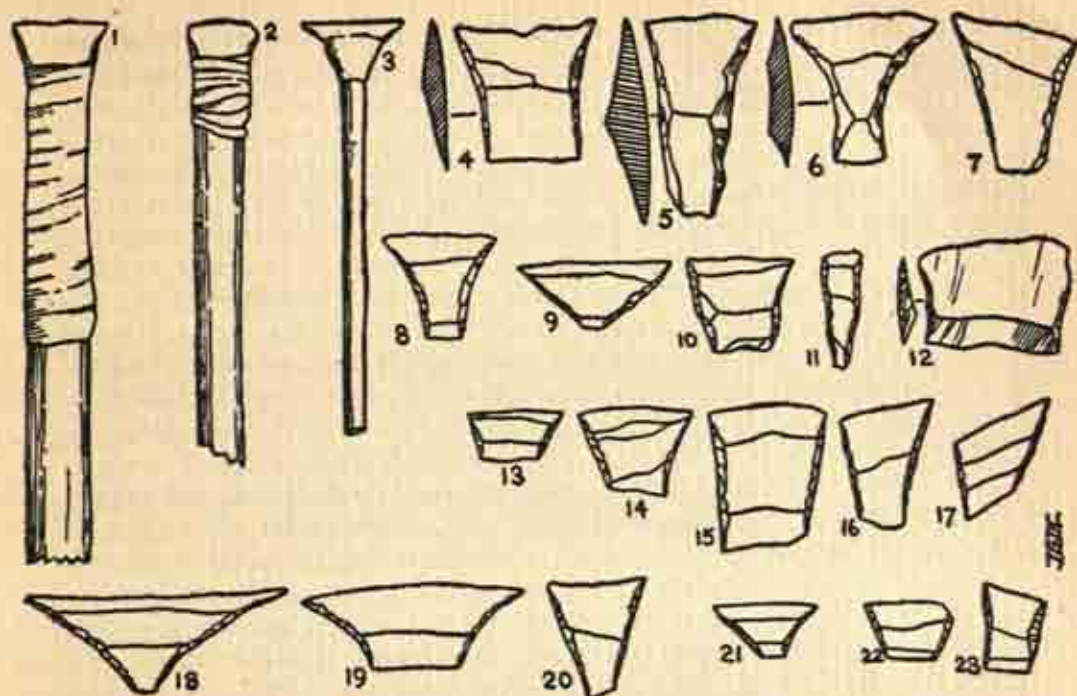


FIG. 2. CHISEL-ENDED TRANSVERSE OR TRANCHET ARROW-HEADS OF FLINT,  
ALL EXCEPT No. 3 FROM THE MESOLITHIC PERIOD.  
Natural size except no. 2 at  $\frac{1}{2}$ ; nos. 9-11 at  $\frac{1}{3}$ ; nos. 13-17 at  $\frac{1}{4}$ ; no. 20 at  $\frac{1}{2}$ ; no. 21 at  $\frac{1}{3}$ .

1. Tvaermose, Eising Sogn, Ginding Herred, North Jutland. After Sophus Müller, *op. cit.*
2. Petersfehn Moor, Oldenburg. After Kossinna, *op. cit.*
3. Middle Empire, Egypt. After de Morgan. *op. cit.*
- 4-7. Kitchen-midden of Ertebølle, N. Jutland. Drawn by author in Copenhagen Museum.
8. Svaerdborg, Zealand. (See note 25). Drawn by author in Copenhagen Museum.
- 9-11. Under sand-dunes at Wangford, Suffolk. Drawn by author in the University Museum of Archaeology and Ethnology, Cambridge.
12. From floor of a Later Tardenoisian pit-dwelling at Selmeaton, Sussex. Excavated and drawn by the author. See *Antiquaries Journ.*, 1934, xiv, 134 ff. Lewes Museum.
- 13-17. From the upper Tardenoisian level at Zonhoven, Belgium. After Hamal-Nandrin and Servais.
- 18, 19. From the Later Middle Capsian midden of Aïn Mouhâad, Algeria. After Menghin.
20. From a Later Capsian site in Algeria. After Menghin.
21. From the middens at Mugem, Portugal. After Obermaier.
- 22, 23. From the Later Tardenoisian site of Slochy Ogródinki, Poland. After Szmit.

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and Gudenaa river cultures, and extremely abundant to the exclusion of other microlithic forms in the kitchen-middens of Denmark. A series extending from Spain to Poland is illustrated by FIG. 2.

The type continued in use and is found commonly in megalithic tombs in Iberia, Brittany, Holland, North Germany and Denmark; it also occurs rarely in the British Windmill Hill culture, *e.g.*, at Whitehawk Camp, Brighton, and at Windmill Hill,<sup>11</sup> and continues into the Bronze Age.<sup>12</sup> It seems also to have given rise to a large number of derivative forms, which are found commonly in Britain from the Neolithic period and on into the Bronze Age.<sup>13</sup> These derived forms have not been well studied elsewhere, though isolated specimens of derived forms have been illustrated from France. A similar development from a transversely to an obliquely hafted form was observed by Siret<sup>14</sup> in Grenada and Almeria, and supported by Breuil.<sup>15</sup>

In North Africa the material is not sufficiently well-dated geologically to make time-comparisons with Northern Europe, but the *tranchet* arrow-head (or microlithic trapeze—the same thing) is found extremely abundantly in the Getulian province of the Capsian culture (Eastern Algeria and Tunis), according to Menghin<sup>16</sup> in the later half of the middle or younger Capsian and in the final stages of the Capsian. In Egypt it occurs in the first pre-dynastic,<sup>17</sup> and in Upper and Middle Egypt survived the foreign influences of the second predynastic period,<sup>18</sup> and became a stereotyped feature of equipment for shooting birds and small game<sup>19</sup>. The hieroglyphic sign, in fact, for an arrow was based

<sup>11</sup> J. G. D. Clark, 'Derivative forms of the petit tranchet in Britain', *Arch. Journ.*, 1934, xci, p. 38, and fig. 3, nos. 1 and 2. <sup>12</sup> *Ibid.* fig. 3, nos. 3 and 3a.

<sup>13</sup> *Ibid.* p. 55. <sup>14</sup> See *Bull. Soc. d'Anthr. de Brux.*, 1924, p. 220 and fig. 6.

<sup>15</sup> H. Breuil, 'Les Subdivisions du Paléolithique supérieur et leur signification', *Congr. Int. d'Anthr. et d'Arch. Préh.*, Geneva, 1912, p. 237, fig. 47.

<sup>16</sup> O. Menghin, 'Die mesolithische Kulturentwicklung in Europa', *Deutsches Arch. Inst. Rom.-Germ. Komm. Ber.* 1927, pp. 162-6.

<sup>17</sup> V. G. Childe, *The Most Ancient East*, p. 77. <sup>18</sup> *Ibid.* pp. 80-1.

<sup>19</sup> Possibly it was also used as a weapon by inferior peoples. Evans, *op. cit.*, p. 369, quotes the case of two sets of wooden figures in Ghizeh Museum from a 6th dynasty tomb at Assiut, showing on the one hand Egyptian troops with brown complexions and armed with bronze spears and shields, and on the other shorter black-faced troops 'armed with bow and arrows only; each (with) a bow in his left hand, and in his right four arrows with chisel-shaped heads of flint'.

Possibly the fact that the type is a very ancient one caused it to be used for symbolical purposes in burials. Is this why such arrows occurred only in the grave of Mes-kalam-dug, and not in the other graves examined in the Royal Cemetery at Ur?



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on the chisel-ended form.<sup>20</sup> Various elaborations of the form were perfected in Egypt, *e.g.*, the remarkable specimens obtained by Petrie from Abydos (contemporary with the 1st dynasty),<sup>21</sup> as well as such devices as hafting three heads on one shaft, or two smaller ones on either side of and slightly below a larger central specimen.<sup>22</sup> It is clear in fact that in Egypt and in North Africa generally the type has had a very prolonged history, stretching away back to the later stages of the Capsian.

There is less published evidence from Mesopotamia, but the type seems to have occurred in the first prediluvian stratum at al-'Ubaid.<sup>23</sup> At Ur it was confined to the grave of Mes-kalam-dug.

The suggestion made by Dr Woolley that the *tranchet* arrow-head is possibly 'one more example of Egyptian borrowing from Sumer prior to the rise of the first dynasty' rests on the false premise that the type 'has not been found [in Egypt] in predynastic associations'.<sup>24</sup> In actual fact, as we have shown, it is extremely ancient in North Africa. In the present state of knowledge it would be rash to decide which of the two areas can claim priority, but North Africa must hold the field on existing evidence. As between North Africa and Northern Europe it is hardly possible to say very much; the type is certainly 6000 and possibly 7000 years old in Denmark,<sup>25</sup> but until more is known of the late quaternary geology of North Africa it seems impossible to date the various stages of Capsian culture with any degree of accuracy. It is certainly significant that the *tranchet* arrow-head seems to have arrived abruptly about the middle of the Mesolithic in Europe, and this has led some writers to suggest that it marks the arrival in Europe of some impulse from North Africa.<sup>26</sup>

J. G. D. CLARK

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<sup>20</sup> V. G. Childe, *op. cit.* p. 96.    <sup>21</sup> See de Morgan, *op. cit.* fig. 41, nos. 4 and 5.

<sup>22</sup> *Ibid.* fig. 41, no. 8.    <sup>23</sup> C. L. Woolley, *op. cit.* pp. 380-1.    <sup>24</sup> *Ibid.*

<sup>25</sup> Out of considerably more than 1000 microliths from the Svaerdborg site, complete enough for certain classification, the only trapeze or *petit tranchet* arrow-head is that illustrated. It is important to note that this was one of the few pieces not found in systematic excavation, and its association is therefore suspect. This type is entirely absent from other sites of the culture period, *e.g.* Mullerup, Holmegaard, Duvensee, Broxbourne, Thatcham. It is therefore unlikely to ante-date the Boreal-Atlantic transition.

<sup>26</sup> *e.g.* Obermaier, *Fossil Man in Spain*, p. 326.



## ANTIQUITY

### CRETE AND EGYPT.

We have been asked by Sir Arthur Evans to give publicity to a footnote in the 4th volume of his *Palace of Minos*, in which he refers to Dr Reisner's *Mycerinus* (Harvard University Press, 1931) pp. 130 ff, and to his (Dr Reisner's) article in *ANTIQUITY* v, 1931, 200 ff, on Stone Vessels found in Crete and Babylonia. We quote from a proof-copy of p. 983 of Sir Arthur Evans' book, corrected in his own handwriting:—

... 'As regards proto-dynastic Egypt, I am glad to find that his results correspond generally with those set forth in this Work. It is the more to be regretted, therefore, that, as regards the prehistoric section (in which "Dynasty O", as representing its most developed aspect, should be naturally included) not only is there a serious lacuna—the entire omission of the class of "cupped blocks"—but a clear misstatement of fact. In *Mycerinus*, p. 133, regarding the bases of the Middle and Late Prehistoric stone bowls it is stated that all the examples are of the round-bottomed "type". But the flat base constantly occurs (e.g. *Naqada*, PL. VIII, 1). The flat base with a moulded edge is found both on the smaller class of bowls (*Naqada*, fig. 942 d) and on miniature examples. Such moulded bases are, indeed, a feature of Middle and Late Prehistoric stone vessels in Egypt and disappear in Dynasty 1. No proto-dynastic bowls of this kind exist. It was therefore reasonable to claim as an import from predynastic Egypt the lower part of a porphyry bowl with a moulded base of an unusual character found in the pre-palatial dumping-ground northwest of the Palace at Knossos (*P. of M.* II, part I, p. 59, and cf. p. 31). A small marble bowl (fig. 942 c.) from *Naqada* (Ashmolean Museum) shows a similar moulding. The same kind of porphyry, with somewhat elongated quartz crystals, occurs among prehistoric vases of the Petrie Collection in the University College Museum (40–60 sequence dating), as was kindly pointed out to me by Dr M. A. Murray.

'As explained above, the general indebtedness of Early Minoan forms to predynastic Egyptian models depends on a whole group of parallel objects found in the same early deposits—not only copies of stone vases, but stone palettes, similar idols, as well as features in costume (including a "sheath" akin to the Libyan) fashions of hair-dressing, and apparently the beehive form of their dwellings. All this is brushed aside by Dr Reisner (*ANTIQUITY*, *loc. cit.*, p. 206) with the aphorism that among primitive races "similar needs and materials are apt to produce objects of similar appearance". The fact is of course undoubted, but it is wholly inapplicable to the present case, relating to



PLATE I



FRAGMENT OF ASSYRIAN SCULPTURED SLAB: A CAMP SCENE (See p. 209)  
National Museum, Berlin

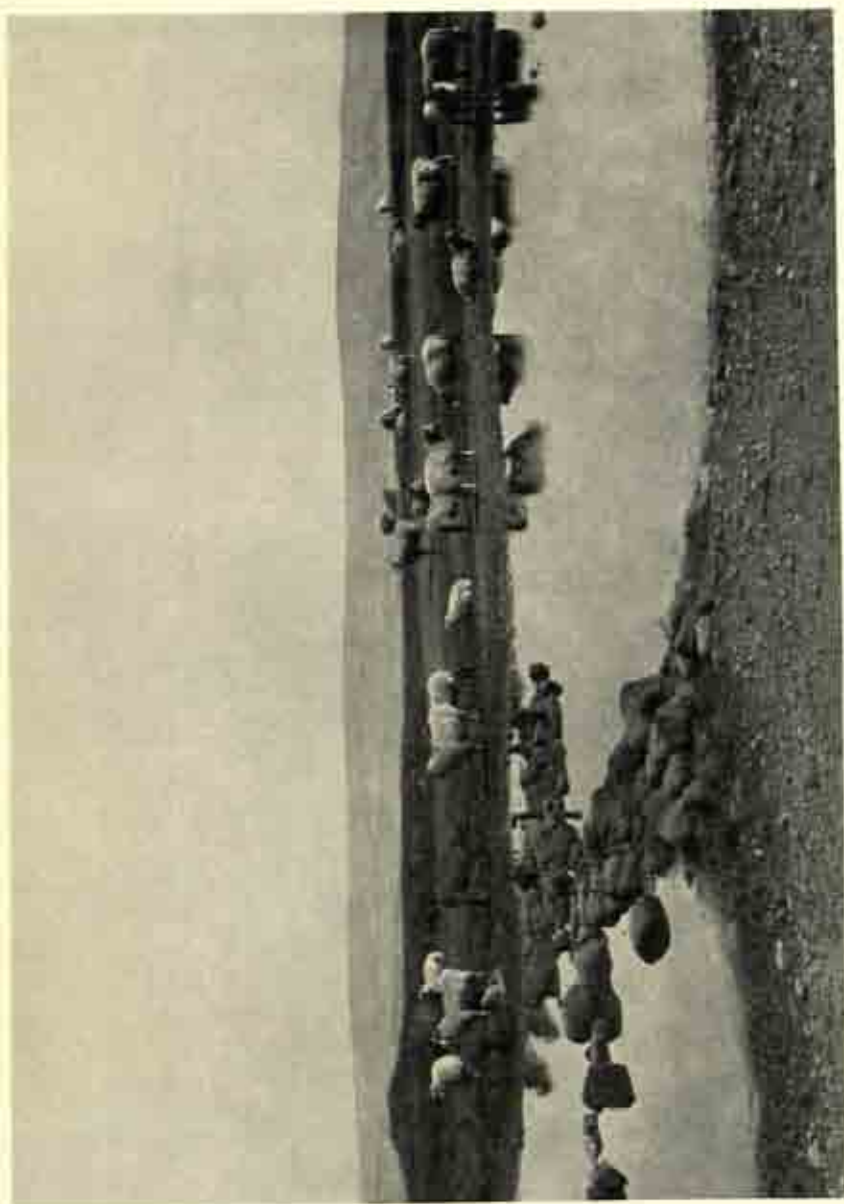
PLATE II



MÉGALITH AT TOBOLWA, KENYA (See p. 240)



PLATE III



TOMLIN TARNS, ST. BEES, CUMBERLAND (See p. 222)  
 Showing the masonry foundations of walls, 3 ft. 6 ins. to 4 ft. wide, built  
 of red sandstone of the district.  
 Ph. Mary C. Farr.





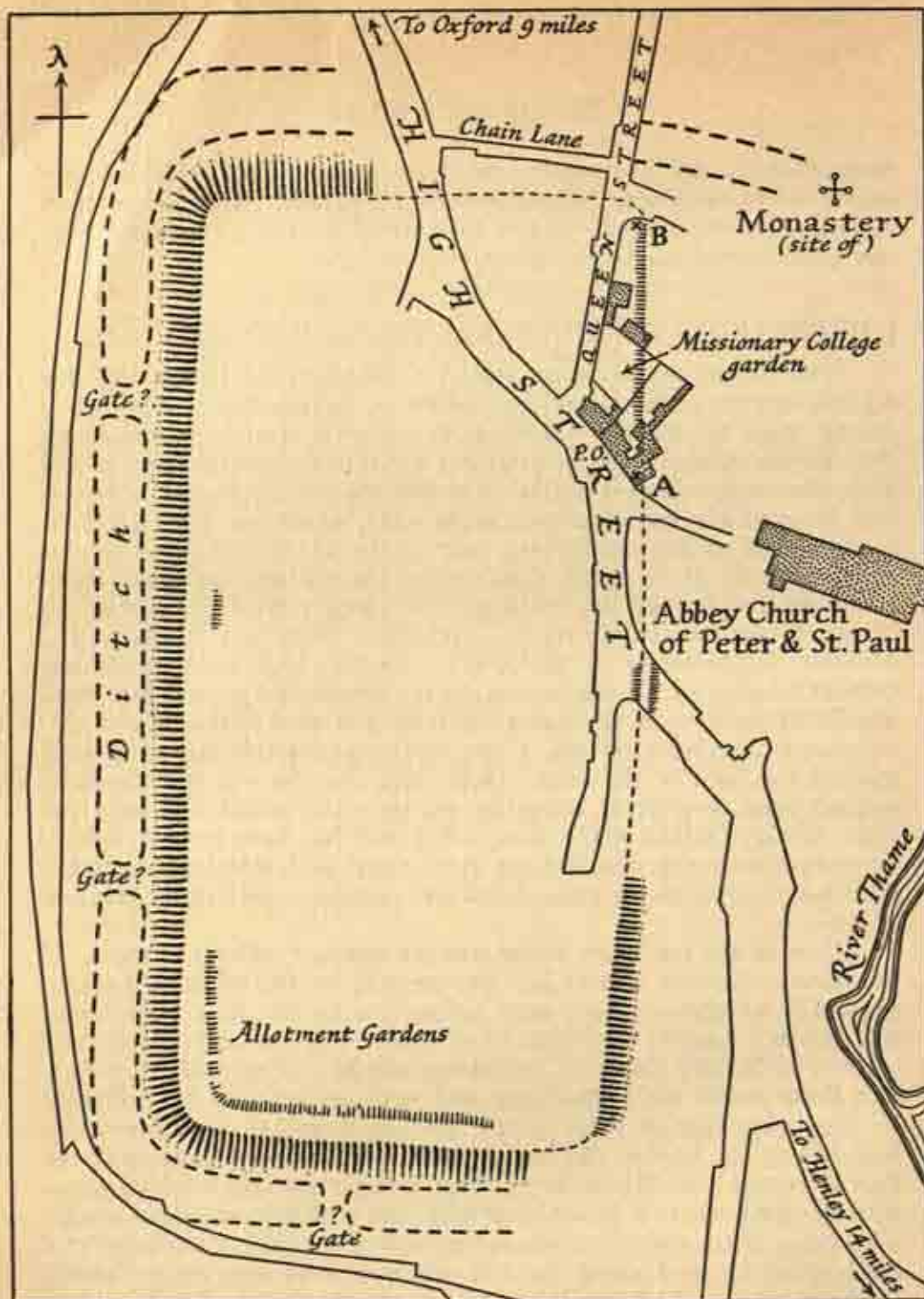
## NOTES AND NEWS

geographically and historically connected areas. The conclusion that under such conditions whole groups of parallel forms of marked individuality were independently evolved without any connexion with one another violates every law of probability'.

### RAMPARTS OF DORCHESTER, Oxfordshire (PLAN)

The *History of Dorchester*, edited in 1882 by J. H. Parker, contains a letter written to the editor by the Rev. Thomas Barns, in which—among other things—he described a 'vallum and fosse' surrounding the modern village. His account is a mixture of field-observation and mere theorizing which it is difficult to disentangle; it seemed, however, that he certainly had seen something and that his account should be tested. The authors, therefore, assisted by G. H. Askew, A. H. A. Hogg, and the Rev. Adam Fox, visited the site and endeavoured to work out the line of the enceinte. For nearly three-quarters of the circuit, it was remarkably plain to the eye. There was a bank much-denuded but standing in places quite six feet high, with slight but evident remains of a ditch in front of it: inside, the ground had risen usually to the level of the top of the bank, but even in the intensively-cultivated allotments on the south side a perceptible rise of ground marked the back of the bank. And once the line was laid down, it seemed even possible to recognize the points at which it crossed the main Henley-Oxford road, though this road has been recently relaid. At two points in the west and one in the south side, interruptions of the ditch were visible on the ground and may possibly represent the position of gates.

Only in the northeast sector was the rampart difficult to trace. It was noted, however, that its line was taken up by two settlement cracks (A and B on plan); these were serious cracks, one in a brick house and one in a modern well-built stone wall. The houses in Dorchester are not noticeably liable to settlement cracks; it seemed, therefore, that these cracks were significant and were probably to be explained by supposing that at these points the buildings had been insecurely founded on the heel of the bank. Furthermore in the gardens of the Post Office and the Missionary College a ridge was visible which aligns with the bank where it is last observable and with the settlement cracks. This ridge, then, should represent the back of the bank. No traces of a ditch could be seen along the east side and there were no settlement cracks in houses which would straddle its supposed line. Possibly it had



Scale of Yards  
50 0 50 100

A and B are Settlement Cracks

RAMPARTS OF DORCHESTER, OXFORDSHIRE



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been filled up deliberately when the Abbey and the Castle of Dorchester were constructed immediately outside it ; an open ditch would certainly have interfered with their amenities ; possibly there never was a ditch along this side ; there was some evidence that both north and south ditches ran on beyond the line of the east rampart to join the Thame.

The area enclosed is approximately a rectangle, 375 yards long and 175 yards broad, and containing about  $13\frac{1}{2}$  acres. Such a rampart laid out in straight lines around a town that is known to be Roman is in all probability Roman itself ; in any event, it can hardly be later than the building of Dorchester Abbey (1140), if we are right in supposing that the abbey is outside it. In fact, it looks as if Dorchester could be added to the list of small Romano-British towns with regularly planned ramparts, of which the neighbouring Alchester (*Ant. Journ.*, 1927, VII, 157) is a conspicuous example : and since Major Allen's photographs have shown that around Dorchester were numerous village-settlements of a large native population, it is interesting to record the ramparts of a town which was, in all probability, their administrative centre.

The Oxford University Archaeological Society is cutting a section across the ditch on the west side, and it is hoped in due course to publish a detailed account of the field-work and the excavations.

C. E. STEVENS ; G. S. KEENEY.

### MEGALITHS IN KENYA (PLATE II, p. 216)

Beyond the not very certain example of Tobolwa, and one other unknown and now unverifiable site, there are no megaliths in Nandi or Vasin Gishu. Of this I am sure—unfortunately, because some megaliths would be a most welcome addition to our rather monotonous structural antiquities. I give a concise description of the three (grand total for Kenya so far) that are known.

#### 1. TOBOLWA HILL, NORTH NANDI

(Top. ref. : Gen. Staff map north A-36)

w

The summit of this hill is a huge mass of granite, with a flat top. On the north edge, the rock has weathered into a semi-circular recess at one spot, and here lies a roughly shaped block of granite, square in section, broken into three pieces. Total length, 10 ft. ; breadth of uppermost face, 36 ins. at one end, and 30 ins. at the other. On the south side of the hill, where the rock does not fall sheer, there is a

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platform of rock surrounding two-thirds of the summit, covered with lumps of rock that have broken off the summit. I have heard it said more than once that there is a stone circle here : but this is definitely not so. There are, however, two blocks of granite which do not appear to be in a natural position. The hill of Tobolwa is regarded by the local Nandi as being haunted. The whole summit, I believe, is to be gazetted as an ancient monument.

### 2. KIBOLOSS FARM

During ploughing operations in 1922, at the foot of Kiboloss Hill, was found a circle of stones, above, and close to, two earth hut-circles. The stones, which averaged about one foot square, were set touching each other in a circle of about 5 or 6 feet diameter. They were removed at the time, as they hindered the plough, and there is unhappily no sketch or plan of them.

### 3. THE SELENGEI STONE

(Gen. Staff map, 1 : 1,000,000, south A-37)

This is at Selenge, about 15 miles southwest of Kiu railway station. It is a cylindrical block of stone, apparently a meteorite, about 2 ft. 6 ins. high, and is said by the Masai to be the figure of a girl who was turned to stone. At the same place are 'Azanian' wells, now disused. The stone is a gazetted ancient monument.

G. W. B. HUNTINGFORD.

## THE OLD ENGLISH TERM 'SNADE'

The term *snade* occurs locally as a suffix to place-names in the southeast of England. It belongs to that class of words : forest, parrock, chase, and the like, which have not only a descriptive but a legal connotation. Its meaning is that of a wood held in severalty, generally within the bounds of Andred or within forest areas under a similar custom of wood-right, near Andred : and in most cases, possibly in all, the woods known as snades were originally royal woods. Thus, in Kent, the weald, or common wood-pasture, of each lathe, had within it, often upon its northern border, a *silva regis* or *cyngessnade*, and these snades have survived in such modern forms as 'Kingsnothe', which was once the 'cyngessnade' of the royal town of Wye, and was given to Battle Abbey by William I together with its parent manor. The severalty which the owner possessed there consisted in the right to fell



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timber, to make assarts, and to exclude the common right of pasturage which the weald once afforded to the Kentish communities in its quality of communis silva. At Wye, and, no doubt, elsewhere, the snade was used to provide wood-right for those tenants on the lord's inland who did not hold by gavelkind, and so had no common right in Andred.

The term is of great antiquity. A king's snade of Faversham is mentioned in a charter of 850 and a snade of the Meonwara (the Hampshire Jutes) in 823, and it is likely that the usage of such royal several woods was coeval with the lathe and weald systems themselves—coeval, I should say, with the settlement.

English charters and surveys show us the legal status of the snade and its fixed relation to the system of forest pasture, but they give us no explanation of the origin of the term itself. To get such an explanation, we must go, against all likelihood, to Italy, and to a period considerably earlier than the first recorded use of the term in English records. The Edict of Rothari, king of the Italian Lombards, was issued in the year 643. It contains, among others, the following provisions under the general rubric 'De Arbore Signato': 'Si servus sua auctoritate arborem, ubi ticlatura facta est inter fines decernendas, incidit, aut moriatur aut redimat se cum quatragenta solidis', and, 'De snaida in silva alterius facta': 'Si quis propter intento, signa nova, id est ticlatura aut snaida, in silva alterius fecerit et suam non adprobaverit, componat solidos quadragenta, mediatatem regi et mediatatem cuius silva fuerit'. The editor of this section of the *Leges* identifies the ticlatura which is here the Latin synonym for snaida with the talia-tura of the Roman agrimensores, the incising of identifiable marks of ownership upon standing timber in order to establish a boundary, and his identification is clearly right. The Lombardic snaida and with it the Kentish snaed should be derived, therefore, from the same root as the Old English snaedan or snipan and the modern German schneiden: in the first instance the cuts made upon trees to mark the lines of a several copse, and, in England, by extension of meaning, the area which the boundary marks enclose. Rothari's edict gives to the lines of blazed trees which form such boundaries a legal guarantee of a clearly defined order—that which attaches to the charter and to the king's coin. The forger of charters, the coiner, and the man who counterfeits or defaces his neighbour's snaida, suffer the same penalty of the striking off of the hand that violated the sanctity of record.

There is nothing in these provisions to show that a system of wood-right like that of Kent prevailed among the Lombards. Rothari's



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edict tells us only that the snade originally meant no more than the owner's mark upon the boundary line of his timber. From this first simple meaning there arose in Kent (but perhaps not among the Lombards) a secondary use. The wood enclosed within the snades or cuttings came itself to be called the snade. Clearly, this use could only arise in circumstances when private property in woodland was a notable exception, when the blazed signs of ownership conveyed a distinctive kind of protection and guaranteed a known and exceptional right. Woodland in the ownership of individuals could only be usefully classed and named as snade-land in a community for whom there was a natural antithesis to it in the common right in national or provincial forests. The snade, therefore, falls into that class of elements in English place-names which carries not only a description of natural features, but also a legal and an historical connotation. It reflects accurately and unmistakably a certain phase of early, distinctive, and narrowly localized custom. Such place-names as Snodland or Kingsnorth recall the form of institutions just as accurately as do the contemporary or slightly later Inlands, Huishes, or Bucklands, and much in the same way as the Chattervilles and Freetowns of the nineteenth century reflect the vaguer and more political motives of their own day. J. E. A. JOLLIFFE.

### A NORTH COUNTRY DEW POND (PLATE III, p. 216)

Dew ponds such as those found on the south of England downlands are not often met with in the northern areas, nor indeed do I know of any made in the typical manner with puddled clay and straw foundation. There is however on the great headland of St. Bees, on the Cumberland coast, a curious pool known as Tomlin Tarn which according to local residents is a dew pond.

This interesting piece of water is shown on the 6-inch Ordnance Survey map of Cumberland, 72 NW, on the South Head almost due north of the signal flagstaff mound and the semaphore hut. It is near the 340.5 ft. bench-mark, no name being given to it on the map. The rising ground of Tomlin is however marked to the northwest.

The pool is thus on high ground of rough, uncultivated pasture-land, placed on a sort of saddle between the highest point of the South Head to the south and Tomlin and the North Head to the north; it is protected further on the north side by a massive dyke-bank of turf such as was common during medieval times as a boundary line between different estates. In the present instance the landowners chiefly



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concerned in this area of St. Bees Head were the Lords of Rottington (a branch of the great Cumberland and Westmorland family of le Fleming) and St. Bees priory. Rottington Hall lies about three-quarters of a mile down the hill northeast from Tomlin Tarn, while the priory is just over a mile east by south down in the valley running up from the sea-shore beneath the massive bulk of the headland. Over the dyke-bank cultivation begins, and there are rich ploughlands on the eastern and southern slopes where the scrubland of the summit of the South Head ends. A sixth of a mile to the west the headland falls away in a ragged cliff, 300 feet to the shore below. A mile and a half to the northwest, on the North Head, the white column of the lighthouse nestles comfortably, and a sixth of a mile due north of this is the point which Mr R. G. Collingwood suggests as being possibly the site of a Roman fortlet or signal station which must certainly have existed somewhere on this headland, which commands not only the Cumberland coast to the south, but also Solway shore and the distant line of the coast of Scotland.

Tomlin Tarn lies in a shallow, saucerlike depression in the rough grass: its bottom appears to be firm and gravelly, not peaty or muddy as pools so often are. It is quite shallow, nowhere more than two feet deep, with no trace whatever of any spring which can feed it though even in continuous drought it is not known to dry up. One St. Bees resident told me he had known the water blown out of the pool by tempest, but quite soon it had refilled to the usual water-level. A modern outfall has been constructed, down the slope to the east-northeast, to supply a cattle trough with water.

Radiating out of the pool are what look like foundations of walls, three feet six inches to four feet in width. What these foundations (which are made of good-sized stones, properly built), represent I do not know, for the history of the whole of the Headland is a series of gaps with here and there a tantalizing shred of story.

Prehistoric man bequeathed to us a polished stone axe or two; we have yet to locate Roman remains, and tradition gives us the discovery in the early 17th century of a giant buried with his armour in a corn-field at Rottington. Charters of agreements between the Lords of Rottington and the priory of St. Bees mention as a landmark a great raise and a great heap of stones which suggest the existence of a big cairn or tumulus, possibly the mound upon which the signal-flagstaff of the South Head has been erected. There is a short section of ancient road and enclosures, more dyke-banks, and some grim shafts and tunnels



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on the cliff edge known as Pattering Holes. During the Middle Ages there was no doubt a beacon post upon this headland where the monks from the priory grazed sheep and goats, the sheep cotes and goat pens being mentioned in the charters. All these things blend into a puzzle which wants patient sorting out. What connexion have any of these remains with each other or with Tomlin Tarn? What in the beginning caused this sheet of shallow water to gather? When? Has it been there since before the dawn of history? Did the Romans or Norsemen or monkish farmers engineer it into being? Careful search along the shore of the pool yielded no remains of any kind to provide a clue. In a wall built of blocks of the red sandstone of the district, there are shaped stones which suggest that they have been voussoirs from some important building, and other stones suggesting their previous use for constructional purposes: this wall divides the rough grazing land of the South Head from cultivated land lower down to south and east, so that the stones noticed may have come from some building belonging to the monks, or they may possibly be of Roman origin. Nor is it possible to say if they can be in any way connected with the foundations of walls which form so intriguing a feature of Tomlin Tarn, that curious pool which never runs dry and upon whose surface no weeds grow.

MARY C. FAIR.

### WHITEHAWK CAMP

It is expected that excavations will again be undertaken this year in Whitehawk Neolithic Camp, Brighton, under the direction of Dr Cecil Curwen, as a preliminary to the making of a new road across the camp by the Brighton Corporation. This will necessitate the clearing down to the bare chalk of a strip, 50 ft. wide, and about an acre in area, across the centre of the Camp, cutting each of the four concentric ditches twice. The cost will be borne by the Corporation, and the opportunity thus afforded of disclosing the internal arrangements of a large Neolithic Camp is one that is not to be missed. It is expected that the work will be undertaken during July. (See *ANTIQUITY*, 1930, IV, 28-32; 1933, VII, 476-7).



## Recent Events

*The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.*

An expedition to Nubia, directed by Mr L. P. Kirwan, has opened a tomb of the 5th or 6th century A.D. at Firka, nearly 100 miles south of Wadi Halfa. One burial-chamber, not unfortunately the richest (as would appear) had remained unplundered, though the roof had fallen, and the great depth had caused dampness which destroyed wood, ivory and even some of the metal objects. The finds consisted of spears, bronze and pottery food-vessels, an elaborate lamp of Byzantine type, a green glass vessel, beads and scarabs. Human sacrifice had been practised, and there was no evidence that the inhabitants were other than pagan. 'Firka seems to have been a centre of some importance in Christian Nubia. In the neighbourhood the expedition has already discovered the sites of three churches'. (*Morning Post*, 23 January, and further discoveries 2 February, and 2 March).



Less likely to produce valuable results is a mission dictated to a Mr Paul Brunton (not to be confused with the archaeologist of that surname) by a 'High Priest of ancient Egypt and by a Greek philosopher of the neo-Platonic school', as reported in the *Sunday Graphic* and *Sunday News* (3 February). Mr Brunton is following the time-honoured pathway to a secret chamber in the Pyramids containing 'incredibly old records which were the originals of Genesis of the Bible', and he has been told by the Greek philosopher how to find a cache of books saved from the library of Alexandria 'maliciously destroyed by Julius Caesar'. The strange thing is that Dr Reisner did actually discover a hidden burial-chamber under the shadow of the Great Pyramid (see *ANTIQUITY*, 1927, 1, 216-18), which had been unaccountably missed by all the cranks and quacks; but he found it by the use of his own sound judgment and by the application of scientific method, not by consulting a dead priest!

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The following particulars of a cemetery found at Troy were published in *Nature*, 2 February :—' The long-awaited discovery of a cemetery at Troy is announced by Dr Carl Blegen, of the University of Cincinnati, in a communication issued by Science Service, Washington, D.C. This find was made by the third expedition of the University to Hissarlik. A second discovery of importance was on a site about three and a half miles from the actual citadel, at a marginal point of the area. Here Dr Blegen found four graves, apparently of neolithic age, containing skeletons which in his opinion belong to a period antecedent to any settlement hitherto found on the site and representing the earliest inhabitants. At the same point, but at a higher level, were later remains, dating from the time of the fourth and fifth layers of the citadel. The cemetery belonging to Troy itself was found just outside the citadel. It is contemporary with the sixth city and consists of a series of urn-burials containing ashes, remnants of burnt bones and traces of ornaments which had not been entirely consumed by the funerary pyres. The practice of cremation burial, naturally and unfortunately, has destroyed all evidence of the physical characters of the inhabitants of the city. A further discovery among the ruined houses was that of a well-preserved buried floor, which affords the first opportunity here for the investigation of a habitation site. The stone bases of columns which supported the upper story are still in place ; but determining the alignment of the columns and the recovery of any household goods which the floor may have preserved will be the work of the expedition's next season '.



It is stated that finger-prints on pottery have enabled fragments from different layers to be equated. (*Birmingham Daily Mail*, 25 January). There seems no reason *a priori* why this method should not be possible if the pottery is hard enough ; and we commend it to excavators to experiment with further.



Prehistoric implements made of glass have been found in the Libyan Desert by Dr L. J. Spencer, Keeper of Minerals in the British Museum (Natural History). We shall look forward to reading his report on these finds and hope that it will not be long in appearing. The



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glass is of natural origin, but its precise method of formation is, we understand, still undetermined. (*The Times*, 28 January and 28 March). It has been suggested that it is the result of the fusion of desert sand by a meteor falling on it. Some of the earliest iron tools were made of meteoric iron (see *ANTIQUITY*, 1928, II, 226-7), and it would be remarkable if some of the oldest stone ones also had a heavenly origin.



Among recent acquisitions by the British Museum is an agate amulet modelled in the form of a frog. It apparently dates from the Agade dynasty of Ur (c. 2500 B.C.) and is the Museum's first acquisition of a first-rate example of the work of the sculptures of the period which produced the Naram-Sin stele in the Louvre. Its technique is however different, and is a new discovery, so far as its period is concerned, the work being performed by rubbing grooves in the hard stone with crushed quartz or corundum, while natural white bands in the agate have been used with astonishing ingenuity and delicacy of workmanship to render the eyes and enforce the shape of the body.



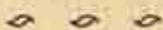
An extensive system of canals and sewers connecting a massive series of buildings now underground has been found at Copan, the ancient city of the Maya in Honduras, by an expedition of the Carnegie Institution of Washington. The buildings include amphitheatres in which are monoliths and large statues in stone. One of the most interesting finds is a pair of solid gold boots, nearly two inches high, of exquisite workmanship. This is important, as with the exception of two doubtful examples no other objects of worked gold have been found in the period of Maya civilization to which the ruins of Copan are dated. (*Nature*, 2 March).



Señor Luis Valcarcel, well known for his excavations in Peru, describes in the *Illustrated London News* (13 April) the mountain-citadel of Pisaj, an almost unknown Inca stronghold. The fortress is a wonderful example of ingenuity in overcoming the difficulties of a site such as the one on which it is built.

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'Woodhenge' at Arminghall, near Lakenham Baths in Norfolk, is to be excavated this summer. It was first observed from the air by the Royal Air Force in 1929, and subsequently photographed by the Norfolk and Norwich Aero Club. The excavations are to be directed by Dr Grahame Clark and Mr Rainbird Clarke. (*Eastern Daily Press*, 28 March).



A summary of the third preliminary report by Dr Henri Frankfort to the Oriental Institute of Chicago on the excavations in 1932-33 at Tell Asmar, Khafaje, and Khorsabad in Iraq is printed in *Nature*, 23 March. The report (price 7s) has been published by the Oriental Institute as communication 17, and also by Cambridge University Press. It presents a valuable description of the discoveries on these sites.



We printed in *ANTIQUITY*, March 1934, pp. 95-96, a note by Dr L. S. B. Leakey on the remains of early man which he found at Kanam and Kanjera in East Africa. It will be remembered that Dr Leakey came to the conclusion that the remains occurred *in situ* in beds of Lower Pleistocene and Middle Pleistocene Age. In a letter to *Nature* of 9 March Professor P. G. H. Boswell, after a visit to the sites and having every opportunity given for studying the geology of the deposits from which the Kanam mandible and the Kanjera skull were obtained, gives reasons, on the grounds of the uncertain location of the sites and also of the doubt as to the stratigraphical horizons from which the remains were obtained, for not being able to accept Dr Leakey's conclusions.



In a letter to *The Times* (6 March, p. 15) Professor G. Elliot Smith records the first occasion of an X-ray photograph being taken of an Egyptian mummy. It occurred in 1903, when he and Mr Howard Carter took the mummy of the Pharaoh Thothmes IV to a nursing home in Cairo where Dr Khayat Bey was in charge of the only X-ray apparatus then in Egypt.



## NOTES AND NEWS

The head of a cult-image, with eyes of shells, has been found in Jericho by Professor Garstang, under a layer of Early Bronze Age deposits 21 feet thick. (*The Times*, 4 April).



In a report of the excavations at Tintagel, Cornwall, Mr Raleigh Radford rejects the association of King Arthur with Cornwall, 'attributing the legends to the later period of the Britons' retreat westwards before the Saxon advance'. The monastic remains found probably date from c. A.D. 500. (*The Times*, 29 March).



The Cumberland Excavation Committee have published their report for 1933. It describes excavations at a number of sites on the Roman Wall in Cumberland. At Birdoswald the investigation of the early phases south of the fort, was continued, and the relation of the multiple ditch system of the stone fort to the Turf Wall and Stone Wall defined. Turrets further west were examined, while at Castlesteads fort excavations determined the line of the fort walls, thus permitting a more accurate estimate of its size. The report is in the *Transactions of the Cumb. and Westmd. Archaeological Society*, volume 34, pp. 120-65.



The place of the Vallum in the development of the frontier works is discussed by Mr E. B. Birley in *Archaeologia Aeliana* (series 4, XI, 146-57) with reference to excavations in the last decade, and a date about the middle of Trajan's principate is suggested.



A summary of the results of the five seasons' excavations at Colchester undertaken by the Colchester Excavation Committee, written by Mr Christopher Hawkes, was published in *The Times* 12 April, p. 17. The dykes have been accurately surveyed and trial digging has confirmed that they are the genuine defences of Cunobelin's city. The Sheepen Farm area has been conclusively identified as the

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site of the city itself and a fourth dyke (Sheepen Dyke) has been discovered. The Committee propose this year to explore the central area of the British city.

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A 'recent event' was made the occasion for a most interesting article in *The Times* (18 April, p. 13) by Sir Arthur Evans on 'News by Fire', in which he traces the use of the beacon in Britain. Incidentally Sir Arthur points out that no systematic survey of the beacon-system exists and suggests that one should be undertaken. The first paper printed in *Archaeologia* (vol. 1, 1770) was on beacons, and others have appeared at intervals in antiquarian journals. The beacons of Hampshire have also been well dealt with by Mr H. J. White, but a complete survey for the whole country would be of value.

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Some remarkable sculptured slabs have been found on the site of the monastery known as 'Gallen of the Britons', in Gallen Priory, near Ferbane, co. Offaly (Leinster). Two of the sculptures are believed to be quite new to Irish art, one, a 6-7th century slab, being possibly modelled on champlevé enamel-work. Another, known as the 'Marigold Stone', is carved on both faces with a 'marigold' design of radiating petals. (*Illus. London News*, 23 February). Mr A. W. Clapham remarked upon the marigold design in his article on Hiberno-Saxon Art in *ANTIQUITY* for March 1934, pp. 43-57.

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An 'elephant' tomb (so-called from the elephant designs on bronze vessels found in it) of about the 12th century B.C. has been discovered on the site of the Shang-Yin capital at An-yang, China, and is described by the Rt. Rev. W. C. White in *Illustrated London News*, 23 March. Some very remarkable Chinese bronzes are shown.

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We have been asked by the President of the Royal Anthropological Institute to draw attention to a proposed anthropometric survey of the



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racial history of Great Britain. The following letter has been signed by twenty prominent anthropologists and scientists :—

' Britain has lagged behind many other countries in carrying out organized research into the racial history and the present physical constitution of its population, though individual efforts of some magnitude have covered parts of the country.

' To remedy this state of affairs and to encourage scientific cooperation, the Royal Anthropological Institute wishes to set on foot a comprehensive survey of the past and present populations of Great Britain.

' The methods by which such investigations can best be conducted are generally recognized. There is involved the arduous collection, reduction, mapping, and interpretation from more than one standpoint of a mass of data by a number of trained workers who can be found if the funds are available. Money will be required :

(1) To give grants to the workers who would take exact measurements of groups of living people and who would measure the skeletal remains preserved in museums and elsewhere ; (2) to pay for the necessary scientific instruments ; (3) to pay the travelling expenses of the workers ; (4) to help towards publishing scientific reports and popular summaries of the results obtained.

' A really comprehensive survey would lead to conclusions of both scientific and national importance, conclusions which would throw light not only on our history but also on sociological and medical questions.

' Donations in aid of the projected Anthropometric Survey of Great Britain will be gratefully acknowledged by the Hon. Treasurer, Royal Anthropological Institute, 52, Upper Bedford Place, London, W.C.1'.



Quite a number of good books have appeared recently. We wish it were possible to publish immediate reviews of them, but it is not. The best compensation we can offer is to quote a few of them here and now. The 'Prehistory of Scotland', by Professor Gordon Childe ; a volume of essays on Cornwall by Charles Henderson, called 'Essays in Cornish History', published posthumously ; 'The Indus Civilization', by Ernest Mackay ; 'Tell el Amarna', by John Pendlebury ; 'The Spirit of London', by Paul Cohen-Portheim. The last is one of the best books of its kind ever written ; it is refreshingly free from any taint of the artificiality which spoils so many books of this kind. It is extraordinarily good.

## Reviews

OLD NORSE RELATIONS WITH WALES. By B. G. CHARLES. *Cardiff: The University of Wales Press Board, 1934. pp. xii, 167 with index. 6s.*

This is a painstaking piece of work, and even if of the nature of a compilation rather than of original research is a useful guide to a difficult subject. For though there is ample proof of Scandinavian relations with Wales, it is remarkable in view of the Norse kingdom of Dublin on the other side of the Irish sea how small is the evidence of permanent settlement in Wales.

The evidence of Scandinavian influence falls into several classes which are wisely kept separate in this book according to historic value. Chapter I deals with the chronicles and annals, English, Irish and Welsh; chapter II with the history of Griffith ap Cynan; chapter III, with the Norse sagas; while chapter IV is devoted to place-names and the very scanty archaeological remains.

The chronicles and annals can alone be considered of historic value in the strict sense of written material by authors (whether known by name or not) whose veracity, bias and opportunities of knowing the facts can in some measure be tested. Their reliability varies considerably, and it would have been useful to the reader, who is likely to be more familiar with the English chronicle than with the Irish and Welsh annals, if the author had given some criticism of his sources and discussion of their relative value. It is generally recognized that the *Aberpergwm Brut* is late, but suspicion is aroused with regard to some of the others when one finds (in the record of an event dated 870) Montgomery called (p. 14) by the Norman name *Tre-faldwyn* (Baldwins town).

There is no evidence of permanent Scandinavian settlement in Wales in these historic sources, unless we regard it (with the author) as permissible to include in the term Wales the old Cumbrian Kingdom of Strath-Clyde. Nor is there in the history of Griffith ap Cynan. This has hardly more real historic value than the Norse sagas, though it is good evidence of intimate relations between the Welsh princes of the last half of the eleventh century and the Norse rulers in Ireland. Inter-marriages were frequent and many of the Welsh princes must have been half Scandinavian. Transfusion of legendary material in both directions was inevitable.

It was only in the Norse sagas themselves that there is any positive statement of Scandinavian settlements in Cymric Britain. The chief sources are the Egil's saga and particularly the saga of the Iomsborg viking Palnatoki (930-990). Here we read of a Norse kingdom in Bretland, ruled successively by Iarl Stefni, Palnatoki, Biörn hin Brezki (the British) and Palnatoki's grandson Vagn. The credit of drawing attention to these references belongs largely to Mr Alex. G. Moffat of Swansea. The author does scant justice to Moffat's pioneering work. Repeated reference is made to his paper in vol. III of the *saga-book* of the Viking Club, but he is mentioned by name only to criticize his treatment of place-names, inherited from Fenton and Laws, and not surprising in a paper written over a third of a century ago. The author's debt is shown by the fact that he copies even the mistakes which escaped correction in Moffat's paper, and gives Palnatoki a wife *Ingibiargar*, which is, of course, the possessive case of the proper name *Ingibiörg* (p. 100).



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Unfortunately we cannot locate the part of *Bretland* in which the settlements were situated. *Bretland* was the Cymric part of Britain in the 10th and 11th centuries and may have been anywhere from Cornwall to Scotland. On the whole it seems most likely to have been in the district of the Strath-Clyde Welsh. The name *Strae(t)-Cled Wealas* is only replaced in the English Chronicle by Cumbra-land after 944 when king Edward gave it up to the Scots king Malcolm. In Lakeland the place-name evidence of Norse settlement is convincing as shown by W. G. Collingwood, whose name, strangely, is omitted from the author's bibliography. We do not know the precise value to be attached to traditional material of the saga type. Its criticism belongs rather to the realm of Freudian psychology than to history. We see at work all the phenomena which modern psychology has disclosed in dream-phantasy and mythology, wish-fulfilment, identification, dissociation and replacement of heroic personalities, to the utter dislocation of chronology. The processes are seen in the history of Griffith ap Cynan. It is even possible that they have been at work in the account of the battle of *Vinheithr* in Egil's saga, which bears such a close relation to that of *Brunanburh*, but ante-dates it. An interesting suggestion of Eddison's is quoted on p. 97 that in Egil's saga details of *Brunanburh* may have been fused with those of an 'earlier and little-known battle in or about year 921'. If this is well-founded it would throw light upon the celebrated and much debated passage in the English Chronicle (MSS. A and F) which states that Edward was chosen as overlord by the Scots, Northumbrians and Strath Clyde Welsh (*Strae(t) Cled Wealas*) in 924, really in 921. In MSS. C and D (which do not make this statement) it is said that in 921 Edward 'timbered a burg at Clede-mutha'. The site of Clede-mutha is debated, but it is formally the exact equivalent of the Nennian *ostium fluminis Cluth*, at the terminal Roman fort of Antonine's wall at the mouth of the Clyde. Edward was evidently faced in 921 by the same coalition that Aethelstan defeated at Brunanburh in 935. (See *Arch. Cambrensis* 1921, LXXVI, 253). It has never been explained what events caused the submission of the Strath Clyde Welsh and their confederates to Edward. Can this have been the occasion of the 'smaller and little known battle' of 921?

The evidence of place-names is discussed in chapter IV. The author justly points out the weakness of the evidence, the inconclusive nature of test words like *thorpe*, *lake* and *slade*, the presence of Scandinavian loan-words (even *by*) in 10th and 11th century English (which may have entered with the Norman conquests in Wales), and the post-Norman introduction of Scandinavian names from Ireland. That there is a strong Scandinavian element in the place-names of the Welsh coast from Newport to Fishguard is certain. But the evidence of pre-Norman settlement is very weak compared with that of districts in Britain where settlements are historically witnessed. Nor does the evidence of place-names in Wales become stronger when inspected in the light of local knowledge. The street name *Womanby* in Cardiff (p. 152) is much more likely, in the opinion of the reviewer, to be *Hunde-manneby*, the quarters of the 'keepers of the hounds', than the 'farm or village of Hund-mathr' (proper name). It is near the castle and old shambles. What could look more Norse than *Turbernesdune* (p. 153) wrongly identified as Tumbledown on the Cowbridge-Cardiff road? Yet comparison of the documents and local knowledge convince the reviewer that it is a misreading of *Carvenesdune*, and is the down above the brook *Nantcarfan*.

One would have welcomed a comparison with the effects of the Norse settlements upon the place-names of Ireland. It might throw light upon the paucity of the Welsh evidence.



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The English dialects of Pembrokeshire, Gower and the Vale of Glamorgan are not discussed. Their evidence might have been valuable, but they have never been systematically investigated by competent philologists. It is probably now too late.

It is clear that a large amount of first-hand local investigation is necessary. The printed documents (like Clark's *Cartae*) are full of misreadings and the original MSS are difficult of access, many being in private hands.

There is abundant Welsh material in the P.R.O. which has never been properly examined. It is much to be wished that someone could be found to endow the Welsh Colleges with research scholarships for this work. The qualifications are exacting—a knowledge of medieval Latin, French and Welsh, a good knowledge of English philology and some training in palaeography. Considering the rarity of such equipment more generous acknowledgment should have been made of the real achievements of amateurs whose work was too often one of lonely pioneering. Even with the advantage of academic training and advice pitfalls are numerous.

A few misprints have escaped correction (apart from the howler *Ingibiargar*). One of them is amusing and instructive as showing how a Scandinavian appearance can be given to a name in spite of modern resources of proof-reading. The name of the late Mr Albany Major appears on p. 3 as *Alanby*!

CLARENCE A. SEYLER.

**JERNALDERSHUS PA LISTA.** By SIGURD GRIEG. Publication of Instituttet for Sammenlignende Kulturforskning, series B, XXVII. Oslo, 1934. [*Williams & Norgate, London*]. pp. 142 and 41 plates. 16 kr.

The plan and structure of the primitive house are now occupying the attention of an increasing number of students, and this book by Sigurd Grieg, describing the houses excavated by Dr H. Gjessing and himself at Lista in the extreme south of Norway, is a contribution to the subject that will undoubtedly prove valuable. The dwellings belong to the 4th, 5th, and 6th centuries A.D., and are rectangular structures of stone and earth with wooden roofing. Readers of Dr Brøgger's book (*Ancient Emigrants*) will remember his suggestion that the first Norse settlers in Orkney and Shetland included folk from the Lista district, whose ancestors, if not they themselves, had inhabited the houses here described. Dr Grieg has, as always, given us a finely produced book abundantly illustrated with plans and plates, and a most workmanlike account of a fine excavation.

T. D. KENDRICK.

**ÖSTNORSKE RISTNINGER OG MALINGER AV DEN ARKTISKE GRUPPE.**

By EIVIND S. ENGLESTAD. Publication of Instituttet for Sammenlignende Kulturforskning, series B, XXVI. Oslo, 1934. [*Williams & Norgate, London*]. pp. 144 and 61 plates. 18 kr.

This is a competent study of a small but extremely interesting group of late Neolithic rock-carvings and rock-paintings in east Norway. It follows a volume in the same series on the carvings of north Norway and the Bergen Museum publication by Bøe of those in the west. Dr Englestad has carried out a most thorough survey, and his technical pages, dealing with the cleaning of the carvings and his methods of copying and photographing them, will be of use to many field-archaeologists. To the descriptive account is added a capable and suggestive essay on early art, though we shall not all agree with the author that these carvings represent a contact-point between the naturalistic and the schematic traditions. It ought to be realized by archaeologists that an accidentally schematic



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result can be, and often is, only a naturalism crippled by uneducated performance, instead of being the product of a definitely distinct aesthetic tendency. Dr Englestad does not, however, press his views over strongly, and his book is a remarkably good one. There is a full apparatus of references and a German summary. The illustrations are numerous and all excellent. T. D. KENDRICK.

**DIE STEIN- UND KUPFERZEIT SIEBENBÜRGENS.** By HERMANN SCHROLLER. *Vorgeschichtliche Forschungen* 8. Berlin: Walter de Gruyter. pp. 79 and 55 plates. 1933.

Transylvania was once the El Dorado of central Europe, and her obscure local museums are crowded with relics that suggest early connexions with Asia and the Aegean as well as with central Europe. Despite a few summaries, notably Nestor's masterly article in the 22nd *Bericht der römisch-germanischen Kommission*, little of this material is accessibly published, so Schroller's book with its 55 plates is quite indispensable to anyone wishing to understand the relations between Europe and the Near East. Nevertheless it must be remembered that Schroller's classifications are in a high degree provisional; for most of the material comes from unscientific diggings so that associations and stratigraphy are doubtful or unknown. In some cases Schroller's groupings are arbitrary: the Wietenberg pottery that he calls 'the oldest native Neolithic group in Transylvania' must on the contrary, as Nestor has seen, be assigned to the Bronze Age; the *Furchenstich* and *Linsenkeramik* of central Transylvania may be contemporary rather with the *Schneckenberg* culture of the Alt valley than with the earlier painted ware of Erösd type against which Schroller elects to plot them. Through ignorance of the East European material (the work was composed before the appearance of Äyräpää's paper on Russian battle-axes, of Schmidt's *Cucuteni* and of *Dacia III*) and that from Asia, the author over-emphasizes northwestern connexions, e.g., in the battle-axes, and uses the clay stamps for an absolute chronology that they cannot furnish. (Still it is something that he admits their Asiatic inspiration!). Finally he is obsessed with the idea of Nordic invaders. To discover them he repeats the absurdity that the rectangular house is necessarily Nordic (as if rectangular houses had not been used in Asia since the earliest mat-hut of the Sumerians!) and divides up the Erösd culture in a way that finds no support in the stratigraphy of the site. V. G. CHILDE.

**DIE NORDISCHE RASSE BEI DEN INDOGERMANEN ASIENS.** By HANS F. K. GÜNTHER. Munich: Lehmanns Verlag, 1934. 7.50 marks.

Professor Günther's numerous writings merit serious attention; for he is a leading exponent of the anthropological creed of contemporary Germany, and the authority cited by Dr Frick, German Minister for the Interior, in his official memorandum on the teaching of history in state schools (translated in *Nature*, 24 February 1934). An integral part of the dogma in question is the identification of the 'Nordic race' with the original speakers of Indo-European languages. The present work is devoted to discovering traces of Nordics among Hindus, Persians and other 'Aryan-speaking' peoples of Asia, and at the same time to expounding the lessons in 'racial hygiene' to be learned from their fate.

On the anthropological side the 'proof' that 'in early times and even today a Nordic aristocracy is traceable among' these peoples, follows lines familiar from Zaborowski and kindred writers of last century. The net result is that certain physical characters



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(sometimes mere longheadedness is cited) that are frequently associated together among some populations of northern Europe today, can be observed sporadically, and perhaps more often in the past than now, among Brahmans, Kurds, Persians and Afghans. Our confidence even in this demonstration is shaken if we check the author's references; nothing for instance in Fawcett's article on the Naqada crania (to which we are referred) justifies the statement that 'Nordic hordes must already (i.e., about 6000 B.C.) have reached Upper Egypt where their skeletons have been found at Nagada'. In any case the number of anthropologists who would infer blood relationship from the sort of characters cited by Günther, or would recognize embodiments of a 'race' (which Günther defines as a genetically pure human stock distinct from all others) in ancient statues or paintings, is today rapidly dwindling as the results of modern genetics penetrate into anthropometrical laboratories.

But Günther's Nordic race was 'distinguished from all others' by psychological as well as physical peculiarities. The former can then be used as well as the latter to demonstrate its presence in Asia, and agreements in habits, in outlook, in religion and in art between Vedic Hindus, Old Persians, or Tocharians on the one hand, and Germans or Kelts on the other, can be taken as manifestations of the Nordic 'race-soul' at work in the Ancient East. It is needless to point out that these immeasurable and largely subjective qualities are not susceptible of the statistical treatment by which the measurable physical characters of racial groups might be sorted out in a mixed population. Unless one accept on trust the metaphysical postulates of the racial theory of history, the cultural agreements on which Günther insists can be explained in other than genealogical terms—like language, by social instead of biological inheritance or as reflexions of given economies. While the author successfully emphasizes the difference in outlook between say Vedic Āryas and the industrial and commercial townsmen of Babylonia, he ignores similarities to that of the peoples of inner Asia, such as Koppers has recently stressed.

In supporting his case by data drawn from archaeology Günther, a physical anthropologist, is handicapped on the one hand by insufficient acquaintance with the results of recent excavation in the Orient, and on the other by a certain hesitation between the divergent views of specialists on European prehistory. He makes no reference whatever to the pre-Aryan Indus civilization, and treats Iran as a virtually empty tract which the Iranians could colonize *en masse* about 2000 B.C. On the other hand Günther sometimes seems inclined to use the painted pottery culture of southeastern Europe as a link between the Indo-Europeans of Asia and Europe; once he speaks of 'an advance of the Painted Pottery from southeast Europe to Musyan and Susa'—a chronological and stylistic absurdity from which a reference to the magnificent work of his countrymen at Erech might have warned him. Yet he regards the Corded Ware folk as 'the founders of the several Indo-European peoples', 'the core of the Indo-Germans'. This view is frankly attractive as far as Europe is concerned, but no traces of the Corded Ware culture are here disclosed in Iran or India.

Incidentally one is somewhat disconcerted to find Sennacharib called a 'Babylonian king'.

V. G. CHILDE.

NEW LIGHT ON THE MOST ANCIENT EAST—the oriental prelude to European prehistory. By V. GORDON CHILDE. pp. xviii, 326. Kegan Paul, 1934. 15s.

After a preliminary account of 'the setting of the stage' the author passes in review the recent epoch-making discoveries in Egypt, Mesopotamia, India, Iran and Syria. Although naturally based upon published accounts for the most part, the book is not



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merely a compilation. The author recently visited the lands described and the sites and museums there, and he writes therefore with the freshness and authority of an eyewitness. The book is vitiated by a long list of blemishes ranging from mere misprints, wrong references (an old trouble) and other slips to a bad index and an inexcusably shoddy sketch-map. None of these in themselves is of much importance, and all can be got rid of in the next edition; but they annoy the reader and they should never have been allowed to pass the proof stage by a careful author.

A table of cultural synchronisms would have lightened the labour of reading parts of the book, which are hardly intelligible in their present form except to a specialist.

These shortcomings can all easily be got rid of. The rest and by far the greater part of the book is so good that it is certain to pass rapidly into a second and other editions, and to be used by students for some time to come. There is nothing else that covers the same ground, nor could it have been written by anyone but Professor Childe, with his vast knowledge, great powers of condensation and almost world-wide range of first-hand experience. One of the nicest points made is the comparison between Rome and Sumer (pp. 133-6). The Sumerians are defined as 'the authors of that peculiar civilization which inspired Hither Asia for two millennia as Roman culture inspired Europe'—a most stimulating and original analogy. It is interesting to note that the unification of Babylonia, a great historical event, was carried out by Sargon, a Semite—we are reminded of the sub-title, with its hint of trouble to come for Nordic maniacs. In this connexion it is amusing to note that the swastika and the cross were religious or magical symbols in prehistoric times in India, Elam and Babylonia, when our own racial forbears were placidly munching whelks on the shores of the Littorina sea (p. 222). Tin occurs in Khorassan, and ingots have been sold in the market at Meshed in modern times (p. 187).

It is always dangerous to make abstract inferences of a social kind from mute archaeological and geographical evidence, but legitimate to do so up to a point. We have nothing but praise for the author's attempts (pp. 178-9, 204-8). From the raw materials here presented will some day be written the history of the emergence of organized social life. It may even be possible to make some attempt in our own time, and no one could do so better than Professor Childe.

O.G.S.C.

ANCIENT EGYPTIAN MATERIALS AND INDUSTRIES. By A. LUCAS.  
(Second edition,<sup>1</sup> revised and greatly enlarged). *Edward Arnold*, 1934. pp. xii, 447. 16s.

If a book similar to this were written for every important and homogeneous archaeological area the task of archaeologists would be made much easier. Mr Lucas, Chemist to the Department of Antiquities in Cairo, has compiled a most scholarly work in which every scrap of evidence that concerns the material used by ancient Egyptians is discussed and examined and a thorough scientific explanation given of each material. As a book of reference this will be invaluable to all archaeologists, for while Egypt seems to have preserved samples of almost everything used in their daily life, other regions at least preserve a reasonable proportion. Mr Lucas has virtually superseded his earlier and smaller work on the same theme.

He describes the methods of wine and beer-making, and the Egyptian knowledge of sugar; he discusses textiles and builders' materials, paints and minerals. Of great importance are his discussions of metals, for his statements are both the latest and the

<sup>1</sup> The first edition was reviewed in *ANTIQUITY* 1927, 1, 497.



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most scientific available. Without paying too much attention to fantastic views he disposes finally of mysterious suggestions that Egyptians cut their hard stone monuments by the aid of some very hard metal, or with bronze and copper tools mysteriously hardened. He explains clearly enough how bronze is hardened by beating and annealing, so that a specimen of bronze with an initial hardness of 136 was after hammering increased to a hardness of 257. But even so it would have nothing like sufficient hardness to carve diorite or granite. These stones he explains, following Reisner, were worked by the simplest and most primitive processes: the harder the stone the simpler the process. For the most part it was simple pounding and abrasion. Bronze in Egypt he thinks was not in full use until the 18th dynasty and the beginning of the Bronze Age he puts at the 12th dynasty. Before that copper was the metal universally employed. The earlier use of bronze in Mesopotamia shows that it is an Asiatic invention which spread to Egypt and Europe.

The working of stone he explains was largely done by abrasion, the tools being of stone used with an abrasive, metallic chisels hardly being used at all. Much extensive cutting was also done with the tubular drill and abrasive sand. He disputes Petrie's theory that the drills were fitted with cutting points of emery or other abrasive, and thinks that the marks inside drill-holes which led Petrie to that conclusion are caused by the accidental intrusion of larger fragments of the abrasive into the drilled hole, so that they clogged the drill. In other words what Petrie thinks are intentional teeth fitted to the edge or sides of the drill are in fact accidental. Nor does he believe that the Egyptians employed emery, which he thinks was too far away for import. The only Mediterranean sources are Naxos and Asia Minor. He proposes the hypothesis that they used quartz sand, which would cut anything as hard as quartz, on the same basis that diamond cuts diamond. His reason for rejecting emery is, he thinks, strengthened by the fact that objects of emery stone are found cut into ornaments and utensils and this to him 'seems a contradiction of its use as an abrasive'. And yet on the same page he quotes the instance of certain South American Indians who cut rock crystal with quartz sand. Surely if the Indians could cut crystal into ornaments with crystal sand, then Egyptians could cut emery into ornaments with emery sand. This is an unexpected logical confusion in an otherwise very clear and lucid section.

He has hard words to say to certain archaeologists in the little matter of a theory of the importation of Transylvanian gold to Egypt. He shows how this hypothesis is based on a chemical assumption that will not hold water. In his catalogue of sources of Egyptian gold there seems no doubt that Egypt could provide all her own supply. His account of methods of refining and plating is of great value.

In the matter of iron also he has some painful surprises for the archaeologist. The early existence of iron, postulated by three reputable authorities, for working hard stones he disposes of rapidly enough. But he also refuses to accept the earliest recorded instance of a substantial non-meteoritic iron object, the famous iron spear-head from Buhen. That iron should have been known and used in this way 400 years before the king of Egypt himself possesses only one iron weapon, as a rarity (Tutankhamen's tomb) is so extraordinary 'that more evidence must be adduced before the date assigned to this object can be accepted, especially as it is practically identical with spear-heads used until not many years ago in the same locality'!

His views on tin are full of common sense. He believes that tin was first discovered by being mistaken for copper ore. It was collected from stream beds in the form of tin oxide, or cassiterite, and he suggests that it was first detected by gold-washers. Its



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metallic nature and heavy weight first caused it to be noticed. He considers that the invention of bronze took place probably in northwest Asia and that one likely candidate is the Kesrwan district of Syria, near enough to Byblos to explain the importance to Egypt of that town. The discovery of tin, in short, was due to the supposition that the heavy nodules of cassiterite were a kind of copper ore. Mixed with copper ore it would have produced bronze unexpectedly: smelted separately it would have produced pure tin which would then have been added to the smelted copper experimentally.

The account of minerals and precious stones is important. But there are some surprising statements. The statement that nephrite is found in the Kwen Luen mountains in Kashmir and apparently nowhere else revives a heresy as old as Schliemann. Nephrite is found in Switzerland, Greece and largely in Austria. These were the sources of Trojan nephrite axes. Jadeite, of which several specimens occur in Egypt, is found, says Mr Lucas, 'only in Upper Burma'! This statement alone makes it essential that he should revise this section. His own caution that finds Naxos too far for Egypt to send to for her emery (although Egypt was trading with Crete from the early dynasties, and Crete is a day's sail from Naxos), and Transylvania too far for gold, should here have suggested to him that Burma is an even more exhausting trip for the unadventurous Egyptian! Or did the Burman come to Egypt?

He states further that the chief source for Lapis Lazuli is in Afghanistan and that no confirmation is obtained of the existence of this stone in Persia, as has always been assumed. Again he sends the maker of the Trojan Lapis Lazuli axe of Troy II on a very long trip to the east, while the artists of Ur would have had to travel only a little less far. Actually Lapis Lazuli is found in Transylvania, Russia and Siberia.

Apart from these few but serious defects the author has given us a most important practical handbook. It will be many years before it is superseded. STANLEY CASSON.

MEASURES AND WEIGHTS. By FLINDERS PETRIE. *Methuen*, 1934. pp. x, 22. 2s.

This small book opens with an interesting preface by the author, in which he remarks, 'We realise from weights how civilisation extended from 5000 B.C. over the widest span of the Old World, and we see how they testify to the community of culture'; and again, 'measures and weights are the great revealers of the community of the world in trade and population'. He describes the method of discovering the units of measure employed, from a consideration of the dimensions of a building, and the variations to be expected. Variations in measures and weights are inevitable. Thus, it appears that a uniform set of four Egyptian weights gave the probable error of any one weight as 3 grains, out of a mean of 149.3 grains (one qedet), a variation of about one in fifty. There is a short paragraph on the royal cubit of Egypt, which was 20.62 inches, with the curious speculation that the diagonal of the square of this cubit (the double remen), which comes to 29.161 inches, 'is almost exactly the natural length of a pendulum which swings 100,000 times in a day; at the latitude of Memphis this would be 29.157 inches'. In his brief paragraph on the northern foot Sir Flinders Petrie remarks that 13.2 inches is the English medieval foot; from that he derives the old mile of 79,200 modern inches. It is interesting to recall the fact that more than fifty years ago, Mr W. Flinders Petrie wrote a paper on 'The Old English Mile' in the *Proceedings* of the Royal Society of Edinburgh. He then found that the mean value of the old mile, derived from the Bodleian Map of England, of about the year A.D. 1300, was 1.265 of the present statute mile, that is about 80,000 inches, sufficiently near to the above-mentioned figure if we consider that the



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value is taken from one map only. Altogether 6 ancient lineal measures, 7 ancient measures of capacity, and 8 measures of weight are dealt with. There is much information packed in this useful little handbook. C.F.C.

**MARATHON AND SALAMIS.** By COMPTON MACKENZIE. (Great Occasions series.)  
*Peter Davies, 1934. pp. 169 and 4 plates. 5s.*

Oddly enough no one has before thought of writing a consecutive account of the Persian Wars for general and semi-popular consumption, without too much insistence on the academic disputes that almost block the path of the historians of these troubled events. No better pen than Mr Mackenzie's could have been chosen for the task. He knows Greece, he knows Greek, and he has made adequate researches into his subject. The result is an eminently readable book in which commonsense gives him much help in dealing with the thornier problems.

Today the lesson of the Persian Wars again comes into prominence. Mr Mackenzie makes some forceful modern comparisons. He reads present conditions into past events. And he corrects wrong impressions. The Greek victories did not save Europe from Barbarism, but rather from its absorption into a Totalitarian State. Persia was the first, and perhaps the finest attempt to transform the whole world into a systematized '*oikoumene*', in which the benefits of supreme organization and perfect tolerance would be conferred upon its citizens. For Darius was a really great man: 'not one of the Greek leaders', says the author, 'in regard to the finer qualities of man, is worthy of being matched against Darius, unless it be Leonidas, of whom, except for that superlative act of self-abnegation, we know nothing'. On the other hand all the Greek leaders gave evidence of corruption, incompetence, petty jealousy, and total inability either to think in terms of Greece or to conceive a really national strategy against the invader. And yet the Greeks won, and by winning handed to posterity a handful of ideals, partly tawdry and trivial, partly permanent and fundamental, which the Persians lacked. That is what makes the Greek victories and the Persian wars into a turning-point in the world's history far more important than any single subsequent event.

This in brief is the gist of Mr Mackenzie's claims, and on the whole he is right. His odd claim that the Irish Rebellion of 1916 is the only gesture comparable to that of Leonidas made on behalf of pure liberty during this century, might be taken seriously if the equation Irishmen-Greeks in matters of intellectual and spiritual culture could be established.

In this short book the author has found occasion to make two quite solid contributions to historical controversy—one that the tactics at Marathon of the 'retiring centre' (not unlike the 'elastic defence' of the Great War) were repeated at Salamis on the sea: and that from that repetition one is entitled to conjecture that Themistocles was a moving force in the strategy of Marathon. The other that at the end of the 6th century B.C. the Spartans were seriously considering a concerted move with the Scythians against Darius in the form of an attack on Asia Minor. This conjecture he bases on the fact reported by Herodotus that a Scythian embassy came to Sparta for that very purpose. Mr Mackenzie would hesitate to reject this as mere gossip. They seem at any rate as good as the innumerable conjectures provided by the professional historians, and deserve to be examined seriously.

The illustrations are uneven in quality. Two vase-paintings and a small bronze give illustration of contemporary warfare. But the dreadful cut of the Plataean serpent



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at Constantinople, surrounded by its Victorian iron railing, is wholly unworthy of the book. The list of illustrations also tells me that there are two maps on the endpapers. By an intolerable oversight on the part of the publisher these maps appear to have been completely forgotten. So that one has to read the tactics of the battles mapless.

S. CASSON.

L'ART BYZANTIN. Vol. II. By H. PEIRCE and R. TYLER. *Librairie de France*, 110 Boulevard S. Germain, Paris, 1934. 208 plates, with 141 pages of introduction and descriptive notes on plates, and map. Each volume, about £4.

All Byzantinists will welcome the appearance of the second volume of Peirce and Tyler's important work, *L'Art byzantin*. To many it will be even more welcome than the first, since, whereas the one dealt with the period of formation only, and contained little that was purely Byzantine, the second volume is concerned mainly with the 6th century, aptly termed the 'First Golden Age' of Byzantine art.

Like the first, this volume is in the form of a collection of plates, each of which is discussed in detail in the first part of the book. The descriptions are preceded by a short introduction. After an historical summary there comes an appreciation of 6th century Byzantine art. The various branches of the art are then considered in turn: mosaic; wall painting, which is of minor importance at this age (p. 33); marble wall revetment; pavements, which, though few have survived, deserve a more detailed consideration than they have heretofore received—that at Sabratha in Tripolis is a superb work of art; sculpture; silver-work, jewels, enamels; manuscripts; and finally, textiles.

It is the last section of the text that is the most important, for it is a great deal more than a mere summary of the subject. The authors provide much new information about early silks, and supplement considerably the studies of Van Falke. In brief, they assign more initiative in this important art to Byzantine culture than previous authorities have done, and they go so far as to say that practically none of the surviving silks of Persian character, such as those bearing the famous gryphon in circle-motive, can definitely be attributed to Persia. Egypt, Syria, and probably also Constantinople, were centres as important, if not more so, than Persia, and work that has been preserved in the various treasuries of the West is mostly to be attributed to the Nearer rather than to the Middle East. The authors distinguish also a number of more detailed groups than Von Falke was able to do, and they hint that one of them, characterized by its elaborate, brightly coloured decoration (e.g. the Vatican stuff with Annunciation and Nativity in roundels) is perhaps to be assigned to Constantinople. After the introduction of silk culture by Justinian, in fact, the capital probably became the most important centre in the Nearer East, though it is only in Egypt, where actual specimens are found *in situ*, that we can be certain as to the exact provenance.

One mistake in the bibliography may be noted—*Guida di Ravenna* is by C. Ricci, not 'D. Talbot Ricci', while *Byzantine Glazed Pottery* is by D. Talbot Rice.

The plates are excellently reproduced and constitute a complete and most enlightening survey, wherein the dated monuments of one art are employed to best advantage to aid the assignation of dates to uncertain ones in other arts. The conclusions which the authors draw are in every case important, and in most instances they seem certain. Three dates may however be questioned. The slab bearing the Archangel Gabriel (pl. 32), which is assigned to the 6th century, has thus certain characteristics of the 9th century, while the busts of the Apostles on the arch from St. Mary Panachrantos at Constantinople



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(one is shown on pl. 15, a) are again quite possibly later, though in this case the later date is less well indicated.

The period to which the Kiti mosaic in Cyprus (pl. 127) is to be attributed is a more complicated question, since evidence as to its 9th century date is just as convincing as that in favour of the 6th century. The colouring, style and composition of the mosaic and the types of faces of the figures—it was not actually seen by Peirce and Tyler—support very strongly the later date, while the iconography and the nature of the inscription support the earlier. But an archaic wording and arrangement might well have been retained through conservatism in a provincial place like Cyprus, while a craftsman who had been trained in a more up to date type of art would be allowed free scope as regarded the actual work. We can only hope to get further with the dating of the early mosaics when the work in St. Sophia at Constantinople has been completed. It is to be regretted that it was not possible to include a photograph of the apse-mosaics in the Sinai monastery, which are definitely of the age of Justinian, but which have never been adequately published.

These are however but three plates out of the 208 that go to make up the book, many of them with several figures on each. The criticisms here set forth are thus of detail only, and the authors are to be heartily congratulated on their work. May the remaining three volumes—if the work can really be compressed within those limits—be speedily forthcoming.

D. TALBOT RICE.

BEITRÄGE ZUR KENNTNIS DER STEINZEITLICHEN MUSIKINSTRUMENTE EUROPAS. By OTTO SEEWALD. Vienna: Anton Schroll, 1934. 12 marks.

We have read this study with the greatest interest, enhanced, possibly, by the fact that we took it up with some misgivings. Though we learnt in Sunday-school that Jubal was 'father of all such as handle the harp and organ' in the days before the Flood, the combination 'Musical Instruments' and 'the Stone Age' comes to us with a semblance of incongruity. Yet this is quite unreasonable. Rhythms—often highly complex—and the varied pitch of sounds give pleasure to the most primitive peoples that can come under our observation. There is no valid reason why the men of the Stone Age should not have enjoyed the like pleasure, and fashioned contrivances by means of which to gratify it.

A modern orchestra is classified into three departments—the strings, the wind, and the percussion instruments. The first of these do not find any prototypes among the materials here brought together, but this is easily comprehensible. Primitive stringed instruments are necessarily made of perishable materials; the lyres and fiddles of the Stone-age Jubals would not have had the slightest chance of surviving to our day in any recognizable form. Their place is taken for us by certain noise (rather than music) making instruments, such as bullroarers, and what are here called *Schrapern*—toothed rods of bone, along which another rod is smartly drawn: an instrument of the same class as the watchman's rattle, or the stick-and-railings combination beloved of the street urchin.

The other two departments are well represented; we treat the fife-and-drum band with a new respect, when we realize the antiquity of its component instruments. Primitive flutes or pipes would, in ninety-nine cases out of a hundred, be made of reeds rather than of the less easily worked bone. The large array of wind-instruments of



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bone which the author has succeeded in collecting thus becomes all the more imposing. Some of these are simple whistles, but others are rudimentary flutes, with finger-holes, and thereby made capable of emitting sounds of different pitch. They are not, perhaps, all equally convincing: and we could wish that the queer figure from the *Caverne des Trois Frères*, explained as a man in an animal mask playing a flute, were a little less sketchy. If the explanation be sound, the instrument must be a nose-flute.

The instruments explained as drums are a well-known central European type of pottery objects. They are (roughly speaking) of a dice-box shape, open above and below. The author would complete them with a now disintegrated skin, stretched over the mouth, and beaten as the skin of a drum is beaten. And with the modern oriental *derebukkkeh*, which is constructed in just such a way, staring us in the face all the time, we can only wonder why we never thought of this before! With his clay drums the author combines bells and rattles of the same material—the latter, certainly, have long been known from excavations in the Near East; and the case that he makes for his theory is most persuasive.

The reader who wishes to profit to the fullest extent by the study of this book will have to prepare himself by a mastery of Menghin's great *Weltgeschichte der Steinzeit*, for the author, a pupil of Professor Menghin, has adopted the complex terminology which his teacher has introduced.

R. A. S. MACALISTER.

THE HADRIANIC SCHOOL: a chapter in the History of Greek Art. By JOCELYN M. C. TOYNBEE. Cambridge University Press, 1934. pp. XXXI, 254 and 59 plates. 42s.

The tendencies of criticism embodied in this important volume are expressed in its title: if it appears at first sight misleading to apply the term *school* to what would ordinarily be called a phase, style or period of art, the use is motivated by a desire to emphasize the common Imperial impulse of the times. Dr Toynbee sees that under Hadrian, and doubtless under his personal influence, the artistic achievements and conventions of the classical and Hellenistic Greeks were applied to the expression of the spirit of the age<sup>1</sup>; and the Roman Empire itself she regards, in many of its aspects, as the culmination of 'Greek' developments and itself essentially 'Greek'. The heading of the first page of text, 'The Roman Empire in Greek History', indicates the angle of vision from which these Hadrianic developments assume their place in perspective as 'a chapter in the history of the art of Greece'. And the 'Summary' at the close of the book opens with the sentences: 'The age which we name "Hadrianic" after a Roman Emperor may be truly regarded as the high-water mark, or culmination, of ancient Imperialism as conceived by Greece—an age in which the Empire was so firmly knit together that the central government could afford, as never before, to encourage interest and pride in local culture, history and antiquities, especially in the cities of Greece and of the Hellenised East. Hadrian was the real author of the "golden age of the Antonines" in the second century; and it was the developed, Hadrianic ideal of the unity of mankind that the third and fourth centuries struggled to keep alive, until the division of the Empire at the beginning of the fifth century symbolised the defeat of that ideal and heralded the final dissolution of the ancient world. So, too, we may see in Hadrianic art the culmination of Imperial art, of Greek art in the Imperial phase'.

<sup>1</sup> For *saeculum* in this sense there is even more exalted authority than that to which reference is made on p. 239: for Trajan's 97th letter to Pliny closes with the words: *Nam et pessimi exempli nec nostri saeculi est*.



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As regards the matters immediately under consideration, the author writes as both archaeologist and historian: her deductions are based on a full study of certain classes of Hadrianic coins, especially the 'Province' series, together with the Antonine 'Province' series—products of 'official' art; and also of certain classes of Hadrianic sarcophagi and altars—these forms of relief sculpture being 'individualistic', 'unofficial' manifestations. And these detailed and original investigations are preceded by an introduction, partly dealing with broad historical questions and partly providing a brief survey of certain classes of 'monumental evidence, outside the scope of the book'.

The value, and the timeliness, of Dr Toynbee's work will be obvious from this outline; it has gone far toward rescuing from neglect one of the most momentous periods of antiquity, and it demands respectful attention for an Imperial Personage whose appeal to our age, owing to temperamental qualities on both his side and ours, has hitherto not been altogether effective. The thoroughness and competence with which this task has been accomplished require no emphasis here: the numismatic section has already received the commendation of Mattingly.<sup>2</sup>

In an age when scientific thought and distinguished writing tend more and more to become divorced, we hesitate to carp at a literary product so admirable as Dr Toynbee's introduction; and yet we cannot resist the feeling that the very felicity with which she has in a few pages presented a résumé and critique of the results of European scholarship during recent decades has tended to induce a false sense of security, and to glose over the serious difficulties that still exist. But, beyond suggesting that the Near East may still have much in store that will eventually be found to bear on these problems, we cannot pursue the theme further here.

The limitations of the book are indicated in the title; for in spite of its considerable bulk it remains 'a chapter', dealing adequately with only some aspects of what it is still convenient to call the art of the Roman Empire, and dealing with these from the standpoint not of Rome but of 'the Greeks', a term which is here given a somewhat nebulous connotation topographically, chronologically and racially. And the inclusion of the Antonine 'Province' series of coins and also the Antonine reliefs of Provinces suggests that the artistic heritage left by the Emperor whose name adorns the title-page was so completely assumed by the immediately succeeding princes as to make the sole mention of Hadrian, though in part justified by his commanding position, somewhat arbitrary. But to express a desire for 'more' is to pay the book a compliment: the high standard of scholarship achieved in the detailed studies, and the many incitements to further enquiry and research provided by the more general discussions, combine to make it one of the most stimulating archaeological and historical volumes that has appeared in recent years. We trust that we may look to its author for further guidance along the difficult but fruitful paths which she has outlined so well.

A. W. VAN BUREN,

**THE HOME OF MANKIND**: the story of the world we live in; written and pictured by HENDRIK WILLEM VAN LOON. *George G. Harrap*, 1933. pp. 506, with 156 illus. 12s 6d.

This book represents the attempt of a literary man to write a text-book of geography that can be understood by everyone, in a style to arouse and hold his attention. It has

<sup>2</sup> *Num. Chron.*, 1934, 138-9.



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the merits and weaknesses that are to be expected from such a venture. It possesses the very great merit, outweighing all else, of being both interesting, arresting and intelligible; one certainly does not have to keep re-reading sentences to discover their meaning. It is full of pregnant remarks such as: 'The Strait of Magellan is of course of great strategic importance; hence the Falkland Islands which guard it are British Territory'. There is a refreshing absence of commonplace cant in his prejudiced description of the ancient Romans as 'gangsters and racketeers who . . . despoiled all of Europe'; and in his regret (when in St. Peter's at Rome) for 'the waste of so much money upon a building that had not a single claim to either beauty or charm'. (A similar opinion was expressed, we believe, by Ruskin).

But it has the weakness, on the scientific side, of occasional lapses that betray an inadequate equipment (though who of us is properly equipped for such a huge task?). It is not true to state (p. 131) that 'your guess is apt to be as sound as mine', if the speaker is Mr van Loon and the reader one who has given up his whole life to, let us say, the study of ethnology or archaeology or geology. Such a reader would never set equal value upon the 'Atlantean' theory and one which associated Basques and Berbers. Then, the chapter on Great Britain contains several gross errors, such as the statement that the Romans 'had not fortified the east coast [of Britain] because it had not been necessary' (p. 221). The Saxon Shore forts and the signal stations of Yorkshire are marked on the Ordnance Survey map of Roman Britain, which is not mentioned. There is no evidence that the Phoenicians ever visited Britain or the Scilly Islands (see chapter v in Mr Hencken's recent book on Cornwall). The description of Angles and Saxons on p. 222 is full of strange lapses and displays an extraordinary ignorance of early English history. The attainments of the Anglo-Saxons in art and civilization are completely overlooked; the continental achievements of Alcuin and Boniface, the apostle of Germany, are not even mentioned. And Bede (who, *inter alia*, tells us where the Angles came from) was a historian of no mean calibre, without a peer in his own time and for long afterwards.

Whether a book that is of such unequal merit deserves to be recommended to the readers of *ANTIQUITY* is a problem that is difficult to decide. Some will find it entertaining; others will instinctively dislike it. But one can forgive much in a geographer who is both human and readable; and the best course to recommend is that the book should be bought and read.

O.G.S.C.

### A HANDBOOK OF GREEK LITERATURE: from Homer to the age of Lucian.

By H. J. ROSE. *Methuen*, 1934. pp. viii, 454. 21s.

Mr Rose writes as well for the student making his first acquaintance with Greek literature as for the more advanced inquirer into detail. The latter will appreciate the convenience of finding, in one volume of continuous narrative, information otherwise scattered throughout various works of reference and now brought up to date; the former, it must be candidly admitted, is not likely to be unduly exhilarated by his new discovery. The author is almost too conscientiously comprehensive; he omits nothing and no one; for example, his careful analyses of the plots of all the plays, extant and lost, of all the tragedians, rob the novice of the thrill of exploration and offer him instead a possibly depressing and certainly bewildering catalogue.

Mr Rose is more concerned with informing us of what the Greek writers were and what they wrote, than of how they wrote. Perhaps this is proper in what is avowedly a handbook, but the glimpses of literary criticism given by the writer (e.g. his remarks



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on the *Oedipus Coloneus* or on Pindar, Herodotus, and Thucydides) suggest that much more of this would have been desirable even at the expense of many of the detailed lists of facts. For instance, the student who knows nothing of Greek literature would not realize from this book that Aristophanes was nearly as great a lyricist as comedian, or that in Apollonius Rhodius may be found the earliest traces of the psychological love-story, or that the Greek Anthology contains some of the most charming vignettes in literature, or that Plato writes some of the most harmonious and urbane and altogether delightful prose that the world has ever seen.

However, if his work be viewed strictly as a handbook, Mr Rose has done good service in collecting and sifting the latest available materials, and his very numerous footnotes, showing the wealth of learning which we expect from him, form a most valuable bibliographical basis for the study of each and every author and subject. Further he is careful to present all sides of such controversies as the Homeric question or the Origin of Tragedy, and to give a sane and considered judgment on them, even if not always acceptable to the reader. There is a useful and interesting introductory chapter on the Greek peoples and their dialects, and a good bibliography and index. The book is of great utility from its very scope; but it is primarily a reference-book rather than a literary history, a *catalogue raisonné* rather than the living story of a beautiful language.

R. G. A.

GESELLSCHAFTSFORMEN IM ALTBABYLONISCHEN RECHT. By DR  
WILHELM EILERS. Leipzig: Verlag von Theodor Weicher. 5 marks.

The late Dr Johns wrote a volume 'Babylonian and Assyrian Laws and Contracts' in which was a chapter on 'Partnership'. That was in 1904. Since then Assyriologists, especially in Germany, have studied the question more nearly. This valuable study by Dr Eilers is the most scholarly to date.

It is to be expected that 'business' law and practice would suffer change during the long history of Babylon and Assyria. Hence the need for a sifting of the material and its arrangement according to time and place. This is what Dr Eilers has done here. His material, none of it new, but some of it recent, dates from the end of the Sumerian dynasty of Ur III to the closing years of the First Babylonian dynasty, and comes from old Babylonian cities such as Larsa, Nippur, and Sippar. Part of the new material is from the recently discovered fragments of the Code of Hammurabi. And in this connexion it is interesting to recall that Dr Johns, in the book referred to above, wrote: 'The Code has nothing to say as to partnership unless its regulations on the point were embodied in the lost five columns'. He was right in his guess. The Code has an explicit ruling in the matter of partnership which will be quoted later.

The word for partnership is *tapputum*. Essentially it was a relationship between a creditor and one, but usually two, debtors. The partnership was in respect of money or of land. Dr Eilers keeps the two distinct and seeks the conditions governing each. He points out that the old Babylonian partnership differed from modern partnership in two respects: (1) the contracting parties were in subordination; (2) the principle: *consensu societas contrahitur*, did not operate: 'Sachingabe ist konstitutives Element der Begründung'. The debtors were, as a rule, commercial travellers. The loan might be recalled at the pleasure of the creditor, or it was a short-term loan for a fixed period. The creditor shared the gain with the debtors, in the proportion of  $\frac{1}{2}$ ,  $\frac{1}{3}$ ,  $\frac{1}{4}$ , according



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as there were one, two or three debtors. But the creditor did not share the losses. The ruling of the Code of Hammurabi was that gains and losses should be shared equally by creditor and debtors. This was in accordance with the Code's general attitude towards debtors, lest, as the Code's preamble says, the strong afflict the weak. But the documents here studied indicate that this particular regulation of the Code did not long, if ever, have reality in law. The winding up of the partnership was effected ceremonially before the sun-god. The creditor might, and sometimes did, demand his share in gold, not in the goods in which the travellers had traded with his money. The debtors had no choice.

In the matter of partnership in land, Dr Eilers shows that the arrangement was that the debtors—the lessees—worked the land. The fruits of their labour were divided between the creditor and the debtor; half each to creditor and debtor; one third each if two debtors; and so on. If it happened that the creditor assumed the position of a debtor, then he shared the work in the field and took an equal share in the produce.

This is a very important, though not a final study of this aspect of old Babylonian partnership law and practice. The key words and clauses are not new, but Dr Eilers' interpretation of some of them is. He would not claim more than probability for it. But the main results of his study seem certain. We may remark that if one thinks back from the period to which these business transactions belong, to the Sumerian period from which we have great quantities of business documents, the impression given is that not only the 'law' but also the terminology of the business is Semitic even though Sumerian ideographs are often used. These later documents seem to belong to a period in which business technique was more developed in a world of expanding trade.

T. FISH.

**HISTORY OF ANTHROPOLOGY.** By ALFRED C. HADDON. *Watts & Co.*, 1934. (*The Thinker's Library*, no. 42). 1s.

This is the second edition of a book first published in 1910, but it is 'virtually a new book'. It is divided into three sections—Human Biology (pp. 1-78, more than half the book), Cultural Anthropology or Ethnology (pp. 79-125), Comparative Sociology (pp. 126-39), a Retrospect (pp. 140-4) and a 'selected index of authors' (pp. 145-6). The history of archaeological discovery (with which alone we should be concerned here) occupies 17 pages (chapter 7). It is we believe the only existing attempt of its kind in existence. The present account suffers slightly, in our opinion, from condensation; for instance, the contribution of Pitt Rivers to the technique of excavation, barely referred to on p. 127 of the first edition, is omitted entirely from the second. The general appearance of the first edition is also far more attractive; the print and illustrations are better, and it reads more easily. Nevertheless the second edition is otherwise both fuller and more up-to-date than the first. The invidious task of selection has been objectively carried out; and the new portraits (Keith, Rivers and Elliot Smith) are well chosen. We found the older book stimulating and readable both when it first appeared and at frequent intervals ever since; and the fact that *ANTIQUITY* and its Editor have the honour of inclusion in the second deserves to be mentioned in this review.

The amount of knowledge and fact contained on these 144 pages is astonishing, and a tribute to the breadth of outlook of its veteran author, himself a living part of anthropological history. Considering its price we hope that every one of our readers will buy at least two copies.

O.G.S.C.



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KENTISH ARCHITECTURE AS INFLUENCED BY GEOLOGY. By JOHN ARCHIBALD. *Ramsgate: Monastery Press, 1934. pp. 54, 75 illus. and map. 2s 6d.*

Mr John Archibald, who is already well-known as the joint author of the East Kent Regional Town Planning scheme, has here written an elementary handbook dealing with the materials with which early Kentish builders had to work. It is an excellent little book, well illustrated by carefully chosen pictures, and it has nothing to lose by the straightforward style in which it is written. A book of this kind is necessarily selective. Frequently it becomes difficult to know what to leave out, but Mr Archibald makes a very fair choice for his readers, and ranging from the more familiar pre-Conquest buildings to the Flemish Renaissance houses that are such a pleasant feature of East Kent, he discusses the qualities of the various materials used in their construction and the devices by which the builders obtained their peculiar effects.

In a future edition, such matters as 'Rhinish products', 'Roman Britains', and 'Clarendon Press' should be corrected; and the particulars of the books listed for future reference are not always accurate.

R. F. JESSUP.

PAINTING IN THE FAR EAST: an introduction to the history of pictorial art in Asia, especially China and Japan. By LAURENCE BINYON. Fourth edition, revised throughout. *Edward Arnold, 1934. pp. xvi, 302, and 42 plates. 35s.*

When the first edition of this famous book appeared in 1908 it opened out new vistas to Western students. To quote the author's words in his original preface, it was 'an attempt to survey and to interpret the aims of Oriental painting, and to appreciate it from the standpoint of a European in relation to the rest of the world's art'. His chief concern was not with questions of authorship or archaeology, but with aesthetic values. China, being the parent of Far Eastern culture, bulked as largely then as in this latest edition. Prior to 1908 not much had been written to foster an understanding of Chinese painting, and the little there was dealt chiefly with details of the artists' lives and archaeological notes. A pioneer work was William Anderson's *Descriptive and Historical Catalogue of a Collection of Japanese and Chinese Paintings in the British Museum*, which appeared in 1886. Thirteen years before that the author had accepted an appointment in Tokyo as Professor of Anatomy and Surgery at the Imperial Naval Medical College. In 1882, soon after his return to England, he sold to the British Museum the paintings which he had collected in Japan. The great majority were Japanese, but some 114 Chinese pictures were included, and the collection formed the nucleus of Far Eastern paintings in the British Museum. Anderson's catalogue laid the foundations for the serious study of the subject. It was an achievement which commands respect, although shortcomings are apparent in the light of present-day knowledge. About the same time M. Paléologue, a French diplomat in Peking, wrote a general survey of Chinese art; and to this S. W. Bushell, for thirty years Physician to the British Legation in Peking, paid fitting acknowledgment when he followed with his well-known handbook, first published in 1904 by the Victoria and Albert Museum. In 1905 there appeared historical treatises on Chinese painting by the late H. A. Giles and Friedrich Hirth, the latter having previously made lesser contributions to the subject.

These provided the historical data which before 1908 were available in European writings. Since then knowledge in the West has grown rapidly. Formerly our



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acquaintance with the artists' work was almost limited to reproductions published in Japan; now there are a few notable examples in our public and private collections to which we may turn for first-hand experience. Probably Mr Binyon himself, chiefly through the agency of this book, has exerted a major share of the stimulus to increased and discriminate acquisition. The Central Asian discoveries of Grünwedel, Le Coq and Stein have enlarged our horizon; and in the cave-shrines of Tun-huang the enterprise of Stein and Pelliot has immensely added to the fund of early Buddhist art. The great mass of material brought back by Sir Aurel Stein to enrich the British Museum and the museum in New Delhi has been made known widely through admirable publications. A photographic record of the wall-paintings in the Tun-huang caves is available in the six portfolios of plates which were issued from 1914 to 1924 as part of the series *Mission Pelliot en Asie centrale*. Unfortunately some of these plates are not very clear; the text has not yet appeared—a matter of keen regret to students, since inscriptions accompanying the paintings provided invaluable clues to iconography and dating. According to rumour, many of the walls have been defaced while White Russian refugees lived in the caves after the War. A complete set of large photographs and some coloured drawings were done at Tun-huang by a Russian expedition some twenty years ago. Not long before his death last year, Dr S. d'Oldenburg consulted me about the feasibility of publishing these, and so presumably they are still in existence. So far as it may be seen from the plates already published, the style of the works in certain caves belongs to the end of the fifth century and to the sixth, and here are the sole examples of Chinese painting known to have survived from this early period. Also recently brought to light, mostly in Ho-nan, are actual paintings dating from the Han period (B.C. 206–220 A.D.). They occur on lacquered objects, bronzes, pottery vases, and large bricks used for the walls of tomb chambers. They show a spontaneity and a vital expressiveness of brush-work which are lacking from those wall-paintings interpreted in stone bas-reliefs which formerly provided our earliest criteria. Recently, too, some of the masterpieces from the Imperial Collection in Peking have been made accessible. The foregoing takes note of only some of the events which have modified the outlook since Mr Binyon's first edition appeared in 1908; there should be added the fact that critical appreciation has been advanced by a number of writers, among whom may be mentioned Fenollosa, Kummel, Petrucci, Sirén, Taki and Waley.

Such were the factors which awoke our consciousness that Chinese artists dominated painting in the Far East, and that theirs was a great art fit to rank with ours in the West. Revised editions of Mr Binyon's book in 1913 and 1923 kept pace with the times, and now in the latest, with his usual insight and charm of language, he brings up to date his survey of Chinese, Japanese, Persian and Indian painting. Certain estimates are modified: for instance, the picture in the British Museum, formerly attributed to Ku K'ai-chih of the fourth century, is accepted as probably a copy done some 200 years later. Another painting, also at one time believed to have come from this artist's own hand, is the *Nymph of the Lo River* in the Freer Gallery at Washington. In 1930 a painting of the same subject was bought by the British Museum for £3500 with the aid of the National Art Collections Fund. Mr Binyon considers it to be a Sung copy, completed by a Ming artist; but I am one of those who assign it to a much later date.

In this fourth edition about a dozen of the illustrations are changed. There are numerous misprints in the text, and the index has more than a hundred wrong numbers of pages, thus detracting from the value of the book for purposes of reference.

W. PERCEVAL YETTS.



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**THE BUCHEUM.** By SIR ROBERT MOND and OLIVER H. MYERS, with the Hieroglyphic Inscriptions edited by H. W. FAIRMAN. (Forty-first Memoir of the Egypt Exploration Society). In two volumes—text and 173 plates.

The Mausolea of the sacred Buchis bulls and their mothers of the Ptolemaic and early Roman periods at Armant were discovered by Mr W. B. Emery working on behalf of Sir Robert Mond early in 1926. It has taken five seasons to clear the site completely. In 1926-7 Mr Emery was in charge; in 1928-9 Dr H. Frankfort continued the work; while from 1929 to 1932 the excavations were carried to their conclusion by Mr O. H. Myers, to whom fell the task of publishing the results, not only of his own work on a particularly ungrateful site, but also those of his predecessors. However well a field-director's records may be kept, it is always a difficult matter for an author to incorporate them with his own, especially when he was not on the site at the time they were made, but Mr Myers has acquitted himself of the task in an admirable manner. An immense amount of work has been put into this report; every detail of interest has been carefully worked up, and the author has wisely enlisted the services of many experts to deal with technical details. To induce such experts, who are not Egyptologists, to contribute to a memoir of this kind requires not only tact but perseverance. This effort is, we hope, the first of many useful additions to Egyptological libraries.

Sir Robert Mond, to whose keen historical interest and munificence the work is due, contributes an introduction in which he records his appreciation of the excellent way in which the excavations have been carried out. His account of the discovery in 1926 does not quite tally with that of Mr Emery in the *Annals of Archaeology*, xvi.

It is a difficult matter to review such a vast amount of detail. The general arrangement of the book is all that can be desired. The historical summary in chapter 1 is a most valuable résumé of the little that is known of the ancient Egyptian bull-worship, and contains some good suggestions. Perhaps some mention might have been made of the wide-spread reverence paid to cattle among the Hamites, both primitive and modern, of which the Badarian burial referred to is only one example, and which is also illustrated by the cattle-burials of the C-group people recorded by Mr G. W. Murray.

All the various classes of antiquities found are described with great thoroughness, and as much information as possible extracted from what at first sight would have seemed to most excavators very sterile material. A small table showing the frequency of occurrence of each kind of amulet would have been useful. The detailed scrutiny which has been given to some classes of objects has perhaps been unnecessarily meticulous; for instance, three plates are devoted to the bronze clamps, about which the author himself remarks that 'these objects were too crudely made to show any development'. The plan of the camp house on pl. xi might also have been omitted; one almost feels that there should have been a little less house, and consequently more work for the same outlay. The sections, plates 172 and 173, are unfortunately divorced from the plans, plates 3 and 4, to which they belong.

Mr Myers' style is vivid, and an occasional humorous touch is not out of place where it helps to enforce a comment. But the footnote on p. 48 is unfortunate; Mr Bagnani's remark was certainly never meant to be taken seriously, nor intended for quotation in a scientific record.

The translations of the inscriptions and ostraca given by Mr Fairman and Mr Mattha in volume 2 form a very valuable part of the book, especially for those who are not able to read the originals. It is however most regrettable that stela no. 16, which has been in



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the British Museum since 1906, and the importance of which was recognized in 1929, should still be so covered with mud that it was not possible to make more than a tentative translation of it for this otherwise complete publication of the Buchis stelae.

GUY BRUNTON.

**LO-LANG:** the Tomb of the Painted Basket and other two Tombs, being a Report of the Excavations in the season of 1931. Japanese text (pp. 150) by AKIO KOIDZUMI; English résumé (pp. 28) by KÔSAKU HAMADA. 133 plates (many coloured): 50 text-illustrations. *The Society of the Study of Korean Antiquities, Keijo (Seoul), 1934.*

Readers of *ANTIQUITY* will remember the account of the Japanese excavations at Lo-lang in northwest Korea given by Mr Sakanishi in his review of the publication of Wang Hsü's tomb in that region (Dec. 1931, p. 521). This is another tomb nearby of about the same date, 1 A.D., not quite so rich and naturally not so much of a novelty, but with an interest of its own. In making it advantage was taken of a natural slope so that hardly any tumulus had to be heaped up: this is probably the reason why the chamber filled with water, which preserved certain things particularly well. When tomb-robbers attacked the barrow, it gushed out and drove them away, but the scientific explorers, in a Korean November, were more enduring. The two chambers were mostly of oak elaborately constructed of logs and planks: they have now been set up on view in the Hei-jo (Ping-jang) Museum: all the details of the carpentry are very well shown in the plates. The bodies and trappings in three coffins were on the whole ill-preserved, but the goods in the outer chamber were better. Perfect is a basket adorned with figure-painting on lacquer, almost our first Han painting and recalling in style the Shan-tung sculptures. Other lacquer objects are cylinders for keeping scrolls, boxes, tables, bowls, trays and shoes: also there was an ink-slab in an elaborate box or stand, hairpins and trinkets, and horses in wood corresponding to the clay horses of China proper. The real designation of the tomb is no. 116 at Nan-sei-ri (Nan-ching-li) a village 2 miles south of Lo-lang, but, the name of the dead man being unknown, it has been called the Tomb of the Painted Basket.

Two other tombs, nos. 201 and 260 at Seki-gan-ri (Shih-yen-li) half a mile to the north, had been plundered; but the former yielded fragmentary lacquers dated 4 and 8 A.D. just like those in other tombs about Lo-lang. Among other things were many of the curious ferrules with side-hooks that have hitherto been explained as arrow-butts, but are now regarded as the ends of the sticks of a great umbrella or canopy. No. 260 yielded an inscribed lacquer and one of the jade sheath-hooks, found as far west as the Volga, that Laufer called *suei*, Professor Yetts (*Burlington Mag.* Oct. 1926, p. 198) *chih*, but the Japanese term *pi*.

The publication is splendid, the work most careful and the English résumé most welcome. We must wish well to the new Society for the study of Korean Antiquities, of which this is the first fruits.

ELLIS H. MINNS.

**THE DESERT FAYUM.** By G. Caton-Thompson, Research Fellow and Associate Newnham Coll., Cambridge, and E. W. GARDNER, Research Fellow Lady Margaret Hall, Oxford, Associate Newnham Coll., Cambridge. Published and sold by The Royal Anthropological Institute of Great Britain and Ireland, 52 Upper Bedford Place, W.C. 1, 1934. Vol. 1, pp. xiv, 167; Vol. 2, pp. viii, 107 plates and 7 maps. 30s.

Although this is the first publication on Egypt for which Miss Caton-Thompson is primarily responsible, she has already established herself as a leader among those who,



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in the face of opposition and enormous difficulties, are trying to convert Egyptology into a science. The volumes under review add much to that reputation.

They are thorough and systematic and exhibit that rare phenomenon, an archaeological conscience, which does not permit the worker to scamp any work, however remote from her original enquiry and interest. Thus, although the main purpose of the work was an investigation of the Neolithic period in the Fayum, nearly half the volume is devoted to a detailed description of historic sites, dating from the Old Kingdom to the Roman period, which the authors excavated in the course of their work.

There is no place in a review to catalogue all the important discoveries of the three seasons' work, but the main subjects studied included the geological, geographical and topographical history of Lake Moeris (Birket Qarun), the Neolithic culture of the lake, the nearby gypsum and dolerite quarries and the working and transport of these materials, the typology of Old Kingdom flint implements, the date and construction of the temple of Qasr-es-Sagha, a Ptolemaic irrigation system, and the so-called quay at Dimai.

The authors disprove the possibility of any artificial lowering of the lake level in historic times, such as has been postulated, and show that its history since Neolithic times has been one of intermittent contraction; the lake was, in fact, 'just below sea level in the earliest dynastic times'. The reclamation work of Ptolemy Philadelphus is shown to have been not the draining of the high level lake, but the construction of those irrigation works discovered by the authors. They prove that the so-called quay at Dimai is 'the free-standing portal of a long processional approach through the town to the temple precincts', and is mentioned in some Dimai papyri of A.D. 200-300.

The graves of the Neolithic peoples were unfortunately not found, and a diligent search for them resulted only in the clearing of several graves of later date. From the carefully excavated and recorded settlements the authors established the existence of two periods, an earlier culture, 'A-group', and a later, decadent 'B-group'. Comparisons are made with the Merimde and the Tasian cultures (Lower, and Middle Egyptian Early Predynastic II respectively), but the connexions cannot yet be fully established. The Neolithic Fayumi was a settled agriculturist, who also hunted and fished along the edge of the lake. He had a highly developed flint industry, made stone vases and beads, wove and plaited and was generally highly developed but for his ceramic industry, which was of a low order. The detailed study of this culture, together with the solution of the problems presented by lake Moeris, form the most important part of the volumes.

The studies of the Old Kingdom flint tools and stoneworking are done with commendable thoroughness and attention to technical detail. On p. 11, Miss Caton-Thompson says 'I have suffered too much from the carefully inadequate publications of some of my predecessors and contemporaries, to have any confidence that we, this generation's archaeological workers, can predict the points which our successors will consider valuable or find useful in our work: a detail, now of trifling importance or none at all, may become the pivot of some future, undreamed of, enquiry. I regret, therefore, not the detail in this volume, but the imperfect handling of what there is, and lack of more'. Such is the spirit in which this book is written, a spirit which must permeate excavators more and more, if archaeology is to take its place on an equal footing with the other sciences.

There are a few unexpected omissions. There is no report by a textile expert on the extremely important fragment of Neolithic linen. An oenological opinion on the vine clippings and other viticultural remains would have been of great interest. No attempt has been made to discover the units of length used in the construction of the various



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buildings measured and planned, nor are the plans dimensioned. Where inductions are not made, measurements should always be given, since the scale of reproduction in a book renders plans valueless for metrology. Dimensions of bricks, etc., are not given with sufficient precision, since the accuracy of recording is not certainly greater than that of manufacture.

On p. 32, 'microphotographs' is used for 'photomicrographs'. Miss Caton-Thompson has adopted the term 'protodynastic' for the first two dynasties: this is not, perhaps, incorrect, but would it not be better to reserve it for the truly proto-dynasties, e.g. '0', '-1', '-2', etc.? On p. 107, the tool-marks on the stone block in fig. 2 of plate 73 are said to be evidence of 'rotatory tooling', which they cannot be, since they radiate spirally from the base of the concavity. Each of the analyses on p. 109 totals to between 100.3% and 102.6%; some explanation of this phenomenon is called for. In one or two places the omission of commas is a source of confusion to the reader.

The format and production are excellent. It is a great advantage to have the plates in a separate volume, though this is impaired by the method of binding, which does not allow the book to lie open. The plans might, with advantage, have been made to fold clear of the volume. The photographs, though for the main part clear, are variable in quality. The plans are beyond criticism and would alone have justified three seasons' work. The drawings are very good, but why have dotted section lines not been included in the pot drawings?

In many places mention is made of sites, which the authors refrain from describing because they are being studied by the University of Michigan, or could not be explored because they lay in the concession of the Oriental Institute of Chicago. It is greatly to be hoped that these will be as well published as the sites studied in these two volumes, for if so they will be given as full justice as can be given to a site today.

OLIVER H. MYERS.

THE PRESERVATION OF ANTIQUITIES. By H. J. PLENDERLEITH. *The Museums Association*, 1934. pp. vii, 71, with 5 figures and 2 plates. 2s 6d.

The importance of this valuable pamphlet is not confined to museum curators; it will be of the greatest service to archaeologists in the field, for often it is the well-intentioned first-aid given to antiquities just excavated that causes the worst damage. In these pages the excavator will learn what not to do as well as what to do. Dr Plenderleith will do a further service if he will be persuaded to publish a brief pamphlet solely for the use of excavators, giving them information on the same lines as in this book, for here the advice given is in fact intended for museum workers. Above all it is important for the excavator to adapt his methods of first-aid according to the climate and soil of the place where he is excavating.

Most interesting is Dr Plenderleith's account of metals. He explains how a patination, if developed in pure conditions, is the best preservative of the metal patinated. Such patinations are acquired slowly and cover the surface uniformly, adding to the weight of the object. All metals (except gold) can be thus preserved by natural processes. But, all too often, impurities, either on the object itself or in the atmosphere or soil, prevent the patination growing evenly. The deadly enemies of such patination are the chlorides which attack iron and bronze above all metals. In iron they appear as small beads of moist corrosion, penetrating the oxidization, and in bronze as small spots of malachite green—the mis-called 'bronze-disease'. If chlorides are still present



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under a bronze patination, they will continue to emerge through the protective surface and ultimately will destroy the patination. Here we learn how to eliminate them. Everything ultimately depends on the history of the object, the soil in which it was found and the climate and region.

Silver presents less serious problems, except for the difficulty caused by crystallization. Crystallized silver can be restored to its original condition by slow heating.

We learn much as to the treatment of textiles, marbles, wood and ivory. In later publications it is to be hoped that the author will give us advice on the difficult problem of the preservation of colour from fading, of the treatment of paint on marble and how to prevent it from flaking, and on the method of removing 'root-marks'.

The author wisely refrains from correlating his enquiries with the detection of forgeries. Such research is better unpublished.

STANLEY CASSON.

ENGLISH VILLAGES AND HAMLETS. By HUMPHREY PAKINGTON. *Batsford*, 1934. pp. 128, with 131 plates, and 27 pen and ink drawings. 7s 6d.

The great difficulty in writing any notice of this book is that it is no sooner opened than one is off on a delightful train of memories of past holidays and sojourns in this or that village, and filled with plans for others in the future. This is a delightful book. The photographs are the work of one with the true landscape painter's eye, and Mr Pakington shares that gift himself; he sees his subjects always as a whole and in their proper setting, and it is evident that his knowledge and affection are very real.

But, when the first pleasure is over and one's errant thoughts have been brought back to the subject, there is a faint, but unmistakable, sense of disappointment. Partly perhaps it is caused by Mr Pakington's style of writing, a mixture of Mr Beverley Nichols and of Mr H. V. Morton; partly by his attitude towards these very villages which, in spite of his affection, he is inclined to look upon as so many pretty trinkets—his only allusions to the *life* of the English village are in terms of Ladies Bountiful and curtsying cottagers. Mr Pakington is an architect, and it is natural that his main interests should be architectural. He classifies his subject in roughly topographical groups according to the geological divisions of the country and the consequent styles of building material, but the scope of the book does not allow these divisions to be more than rough ones, and one cannot help wishing for a more detailed classification on the same lines, showing more of those innumerable and fascinating local idiosyncrasies which give so much charm to rural architecture. But most of all it sets one wishing for an analytical study of the structure and history of English villages; a study which would distinguish between the scattered and the nucleated, between the site occupied in prehistoric or Roman times and that first inhabited in the Anglo-Saxon period or later, the cluster of cottages round the manor house and church from the isolated farmsteads of the north and west. Meanwhile let anyone who is tied to the town acquire this book; he will find himself transported to the country as soon as he has opened it, and to the some 400 villages treated of by the author he may add others from his own knowledge, and indulge in the pleasant occupation of constructing alternatives to Mr Pakington's two lists of the fifteen best and the fifteen next best villages. By the time he closes it, in spite of disappointments, his feeling should be one of gratitude to publisher, author and photographer.

E. G. WITHEYCOMBE.



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**THE PROGRESS OF MAN:** a short survey of his evolution, his customs and his works. By A. M. HOCART, late archaeological commissioner, Ceylon. *Methuen*, 1933. pp. xvi, 316. 7s 6d.

The purpose of this book should be sufficiently indicated by its title, while the need of such a survey is becoming more and more urgent for workers in the sciences that deal with mankind. 'Masses of facts' says the author in his preface 'threaten to crush the student, as they are crushing the specialist. We must organize our thoughts to cope with them, or they will annihilate thought'. But in addition the author makes clear how wide a field has to be covered by defining his own standpoint as one of 'that tiny minority that uses savages and antiquity merely to throw light upon ourselves . . . Indeed I have made a point of linking up the past and the world with our own British present'. Clearly, with such a gigantic aim, success is not easy: nor is there any shame in having fallen short: to have made the attempt was worth while. Moreover, in what Mr Hocart has done there is much that will prove of use to students. But the purpose of the book is not fulfilled; and therefore the main question is to examine what it was that prevented at least a measure of fulfilment.

In the opening chapters the author deals with the genus *homo sapiens* and uses this biological prelude not only for the statement of familiar scientific methods and their accredited results but also for a much needed exposure of the wild speculations and hasty generalizations that are 'still the fashions among psychologists and anthropologists'. Thus for example he says 'It has been rashly concluded that the child repeats *exactly* the mental evolution of the race, that primitive man, and the savage, as his supposed modern representative, has the mind of a child. This doctrine has actually been embodied in the Treaty of Versailles'. When he approached the main body of his work, the author rightly emphasizes the importance of field-work and, in connexion with the technique of Rivers, reaches the conclusion that 'no field-worker can fail to realize that the culture he is studying is one indissoluble whole, that caste, ritual (under which Hocart comprises magic and religion), trade, manufacture, agriculture, are all interwoven and cannot be separated'.

Mr Hocart, having proceeded thus far sure-footedly, abruptly abandons the standpoint he has built up, and for the remainder of the book relapses into the separated treatment of human culture against which he has ably argued. In a score of chapters under headings that are only too familiar in recent years (*e.g.* Quest of life, Quest of food, Sacraments, Power, Disposal of the dead, the Soul) he shuts up the activity of mankind into more or less unrelated compartments. The result is as might have been expected from the method pursued: a useful enough summary is given of various modern researches, but the drawing together of them is not achieved. There is even a confusion of specific differences.

Yet more than once, and especially in the early chapters, the author comes very near to hitting upon methods that would have enabled him to fulfil the stated purpose of the book. When he reached the standpoint quoted above of a given culture as 'one indissoluble whole', he might have gone on to examine what in a culture is the relation between agriculture and industry on the one hand and social structure and kinship on the other, in what way each and all of these are related to caste and ritual, whether any of them appear to be basic and any derivative, and what is the reaction again of the derivative upon the basic. If in any one such 'indissoluble whole' the author had pierced below surface appearances, with their normal separateness, and had found inner connexions, not only of simple cause and effect, but of consequence in turn becoming cause, then a great advance would have been made.



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Then the further question would arise of how a culture ('one indissoluble whole') changes and develops into another culture. This is the process of history, to which the author pays little attention once he has plunged into the separate technological histories of chemistry, of the use of fire, etc., and then into his studies of the soul and the sacraments and so forth. Already in his *New Light on the Most Ancient East* (unfortunately published\* too late for Mr Hocart to use) V. Gordon Childe has given the picture of three related and contemporary city cultures of Egypt, Sumer and Mohenjo-daro, developing from agricultural communities, which in turn he considers had developed from still more primitive cultures. Here, over one and the same large area, are remarkable successive changes in the life of mankind—Childe speaks of them as 'two great revolutions'. To discover the why and wherefore of such changes of culture as these, to combine for this purpose the results of field-work in archaeology with the aid of other disciplines, from climatology and botany to the social sciences, is a necessary part of any attempt to survey the evolution of man, his customs and his works.

Then more involved questions would arise of changes within what to some appear on the surface to be single cultures. Take what is perhaps the best known, though not necessarily the best, example. To the Greeks it appeared that their culture was a continuous whole. Alexander the Great, crossing into Asia, made libation to Poseidon in the mid-Hellespont; on the plain of Troy he crowned the tomb of his ancestor Achilles; and to the Greeks this was as it should be. We know that the Greek culture, appearing to themselves the same in essentials, was yet not the same, and that a gulf of great change lies between Homeric times and the Attica of Solon or the slave civilization of the time of Aristotle. What basic changes had taken place in that culture? 'Diffusion from Egypt' provides no answer here. Only a painstaking scientific study of documents and remains that have so long been the playground of scholasticism, only their study on the basis of the 'indissoluble whole' will bring us towards the answer.

These questions have not been asked by the author of this book. Yet in his opening chapters he gave himself the opportunity at more than one point of a line of advance in his investigation which might have led him to these questions. Thus he says 'The big brain of Palaeolithic man is a safe indication of high intelligence . . . It is quite possible that all progress in culture since the Late Palaeolithic has been due, not to better brains, but to greater specialization, a score of brains dividing between them the work that used to be done by one, in other words, to better social organization'. Again, when he begins to discuss man as a tool-making animal, he might have followed out the remark of Charles Darwin upon the 'extremely unimportant' physical changes undergone by European settlers in America that 'they are completely negligible in comparison with the innumerable changes experienced by the artificial organs of the Americans'. For surely the making and use of tools has had a decisive influence on the development of mankind. By means of tools, chiefly, man carries on a metabolic process with nature. He changes nature outside himself and in so doing changes his own nature. The relation between men engaged in this process, the degree of specialization in any human society, is also bound up with the tools used in this process.

Other aspects of culture follow and are interwoven with the process of material production. But unless the fundamental and continuing significance of man conceived as a tool-making animal is grasped, there is no possibility of a satisfactory survey of human cultures.

R. PAGE ARNOT.

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\* Kegan Paul, March 1934.



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# Antiquity

## A Quarterly Review of Archaeology

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### Editorial Notes

IT has been said that 'all attempts at imaginative creation are merely rearrangements and combinations of known facts'. The facts of archaeology are stored in museums, where they may be arranged according to any one of several systems—geographical, chronological, or both. Or 'imaginative creation' may select and arrange specimens so as to illustrate either the evolution of some special craft, such as fire-making, pot-making, fire-arms, or a subject, like magic.

❧                      ❧

There is no need to appraise the relative merits of these systems, for each has many, and there is—or should be—room for all in a civilized country. After conservation, the one thing essential in a public museum is that it should interest the public and be intelligible to them, and not misleading. It may be opportune to set down some impressions of recent exhibitions, temporary and permanent, leaving to others the invidious task of drawing conclusions.

❧                      ❧

In the Museum of the Trocadero at Paris there has been held a most interesting temporary exhibition, arranged by Monsieur Vayson de Pradenne, designed to illustrate the methods of hafting stone implements. The outstanding feature of this exhibition is that it is



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arranged in accordance with the strictly anthropological method of Tylor and Pitt Rivers ; that is to say, that modern primitive devices are used to elucidate the culture of prehistoric man. For instance, one of the problems of prehistoric archaeology is—how did palaeolithic man hold his 'hand-axes' (coups-de-poing, bifaces, bouchers) ? Did he mount them in a handle, or merely use them unhafted in his hand ? Monsieur Vayson de Pradenne demonstrates, by selected specimens, that the butt-ends of Chellean axes were generally left unworked ; but he suggests that when (as in the subsequent Acheulean period) the butt-end was chipped to an uncomfortably sharp edge, some such substance as gum or clay was stuck on to provide a grip and prevent the sharp edge from cutting the hand. A stone axe of palaeolithic type from California, with just such a gum 'empoignure', is exhibited to illustrate this possibility. It is of course legitimate for anyone merely to guess at the possible uses of an implement he has never used ; but it is far more convincing thus to cite parallel instances from living, or recently extinct, cultures.

The exhibition also contains specimens to elucidate the hafting-methods of stone adzes, clubs, mallets, net-sinkers and anchor-weights (often indistinguishable from throwing-stones and even miners' hammers), notched and other knives (with instructive Eskimo parallels); spears and arrows; and a most interesting collection of agricultural implements, such as sickles, digging-stones and tribulums.

Space forbids any account of the permanent exhibits of the Trocadero, which has recently developed into a museum of the first importance, abounding in suggestive anthropological parallels, such as that between the modern church paintings of Abyssinia and the Esthonian woodcuts of precisely similar religious subjects. Archaeologists will profit greatly by a visit. (The museum is open throughout public holidays, including any Monday that may be included).

The custom of holding temporary ' topical ' exhibitions has recently been adopted by the prehistoric department of the British Museum. At the moment of writing there are two, closely connected—one illustrating Mr C. W. Phillips's excavation of the Giants' Hills long



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barrow at Skendleby, Lincs.; and another of neolithic pottery from Yorkshire long barrows, collected and arranged by Miss N. Newbiggin. Both are full of interest. One is struck by the great advance of technique since Canon Greenwell's time; making possible the detection of traces of hurdle-work in a damp chalk mound 4000 years old, and the exhibition of a finished model of it within six months of its excavation. The use of plans, models and photographs, both here and amongst the permanent exhibits, is a welcome new departure, and one that is obviously appreciated by visitors.

With the Trocadero still fresh in mind one passes hurriedly and with averted glance through the Ethnographical Gallery; ashamed that the anthropological wealth of a mighty Empire (or is it a Commonwealth?) should have to be crowded into a room no bigger than a small suburban garden. One crosses a wooden bridge and enters a clean room full of light and modernity, devoted to the dawn of civilization in the East. Here in the Babylonian room are displayed, amongst other things, the glories of Ur which have just been so finely published by Sir Leonard Woolley. Against one of the walls are no less than seven cases of prehistoric painted pottery from Arpachiyah, Jemdet Nasr and the lowest levels at Nineveh.

The wealth of material and the skill here displayed is in striking contrast with the poverty of the adjacent land of Palestine. Year after year the ancient sites are searched by optimists, but only rarely do the results justify the labour invested. The discovery of inscribed potsherds of the period of Jehoiachim (608-597 B.C.), recorded in the *Illustrated London News* (6 July, p. 19), is a notable event, connecting the archaeology of Tell Duweir with the recorded history of Lachish. These finds, and others, have been exhibited at the Wellcome Research Institution (183 Euston Road, London). The site is being excavated by Mr J. L. Starkey, Director of the Wellcome Archaeological Research Expedition to the Near East.

Not the least interesting remains are the caves, which were inhabited during the third millennium, and when subsequently abandoned were used as burial-places. Is it possible that here in Palestine and Syria

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there may be found a continuous evolution from the natural cave of mesolithic and earlier times (which was used by the living and the dead), through these rock-hewn dwellings and tombs to the megalithic imitation-cave (if such it was, as we have long suspected), erected above ground? The possibility is worth bearing in mind, if only as a working hypothesis; for, if verified, it might help to explain the megalithic sequence on our own western Atlantic sea-board. We shall look forward to seeing the published accounts of the excavations.

Finally, at the Burlington Fine Arts Club was exhibited a *recherché* collection of museum specimens of primitive art (admission was 'by invitation' only). The effect of seeing so many primitive masterpieces in one room was rather disturbing, like a visit to a mental hospital. One turned with relief to the Eskimo ivories, which at any rate display the healthy naturalism of the hunter. None of them however can equal in excellence the scratched scenes of Eskimo life done on a walrus-tusk for Lenin, but which somehow failed to reach him and is now exhibited in the Trocadero at Paris.

The term 'primitive' is of course correct according to current usage; but one wonders whether these horrible idols from the marginal Pacific and African wilds are *really* primitive, or not rather degenerate? In this connexion Monsieur Vayson de Pradenne's theory is of interest (see pp. 305-10). When we find examples of art which is primitive in the literal sense of the word, as on the walls of caves in France and Spain, and on African rock-shelters, that art is, by universal consent, neither crude nor repulsive.

An excellent idea was to leave on the table a folder with press-cuttings. It was interesting to read them after a first inspection of the cases, and then to have another look at some. If only a similar practice were possible in the book world! Of the criticisms, that of Mr Frank Rutter (*Sunday Times*, 2 June) struck one as the best, that is to say, the one that coincided most nearly with one's own.



## The Racial Question—Theory and Fact\*

by JULIAN HUXLEY and A. C. HADDON

**R**ACIAL problems are among the urgent actualities of twentieth century politics. But as soon as we subject the concept underlying them, that of *race*, to dispassionate analysis, it turns out to be a pseudo-scientific rather than a scientific term. In other words, its use implies an appeal to the accuracy and to the prestige of science; but on investigation it turns out to have no precise or definable meaning, and it can then, like other pseudo-scientific terms, readily be employed to rationalize emotion, and to bolster up the appeals of prejudice, by giving it a meaning to suit the context.

The term *race* is currently used in several quite different senses. In the first place it is used to denote one of the major divisions of mankind—black, white, yellow and brown. Secondly, it is used to denote the actual human stock of a particular country or nation and its biologically transmissible characteristics: for instance even the most ardent British upholders of the Nordic theory cannot mean by the 'British race' anything more than the actual inhabitants of Great Britain and their descendants overseas. Thirdly, it is used to denote a hypothetical 'pure race' which is taken to have existed in the past and to have become later somewhat contaminated by admixture with alien elements. This lies behind the idea of 'the Germanic race', for instance. Fourthly, it is used to denote a local population which by reason of isolation has become fairly uniform and stable in physical type—for instance the Veddas of Ceylon.

It is also sometimes used in a wholly inadmissible sense to denote the peoples who speak a certain type of language, for instance in the phrases 'the Aryan race', 'the Latin races', etc.

On all these uses, scientific analysis, backed by the results of modern genetics, throws a pitiless light. It is probable that our species, during its early evolution, became divided up into geographical varieties, each more or less isolated from the others, and each evolving so as to become adapted to its climatic environment. The black variety

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\* This article will appear as the concluding chapter of 'We Europeans' by J. S. Huxley and A. C. Haddon, to be published in September.



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adapted to hot climates, the yellow variety to dry conditions, and the white variety to north-temperate latitudes, are the most prominent examples. Such varieties would then correspond to the 'geographical races', or subspecies as they are now generally called, to be found in many animal species. If we wish to retain the term *race* for such groups, they should be called 'primary races'. However, since the term *race* has been largely abandoned in zoology, and since in anthropology it is used in such a confusing multiplicity of senses, we had better employ the term *primary subspecies*. But—and this cannot be too strongly emphasized—such primary human subspecies are entirely hypothetical, a matter of inference only. Man's incurable and increasing propensity to wander over the face of the globe had effected a thorough mixing between the hypothetical primary races long before the dawn of historic period, and blurred the sharpness of their outlines and in some cases made it all but impossible to deduce their original type. A typical white man is very different from a typical Chinese or a typical negro. But white is connected with black and also with yellow through every gradation of type, and in each case along several distinct main lines of crossing. Again, the simple classifications which at first suggest themselves all break down at one or another point. For instance, the Australians, though black-skinned and undoubtedly primitive in many ways, show the same character of hair as Europeans. We cannot assign them with certainty to a definite original subspecies. No single scheme of classification, in point of fact, has been devised which will provide a satisfactory pigeon-holing for the various human types in existence.

It is here that genetics steps in. The modern study of heredity, based on Mendel's great discoveries, has shown that after a cross between two distinct strains, blending inheritance, such as used to be assumed by anthropologists, does not occur, but that all kinds of different combinations of the original characters of the two stocks are brought into being. This is due to the fact that hereditary transmission is by means of discrete living particles, called *genes*, which reproduce themselves, and can be recombined in the most various ways.

Accordingly, if two primary races meet and mix, their characteristics may be combined in their descendants. Originally, black skin may invariably have gone with flat nose and thick lips. That, however, provides not the slightest reason why it should always do so. After a cross with a stock with light pigmentation and delicate features, the combination of dark skin and delicate features may occur. Since dark skin is an advantage in low latitudes, such a new combination of



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characters has, it appears, been favoured in several instances of actual crossing, notably for instance in India.

Similarly, yellow skin and round head seem to have been originally associated. But the two became uncoupled with the greatest facility after crossing with strains possessing different head-form and skin-colour.

Another result of the existence of definite genes as the carriers of hereditary constitution is that after a cross the resulting population will not tend to a mere average between the two original ingredients, but will, in the absence of social or natural selection, continue to produce a great diversity of types, generation after generation. There is not the slightest tendency for the population of Britain to become all medium brown in hair-colour, nor for that of Germany to become all medium long in skull-shape. In each generation dark and yellow hair, long and round heads, continue to be produced, and to be produced in about the same percentage of the population.

Now it will be clear why no single scheme of classification can satisfactorily pigeon-hole all known human types, or even deduce without danger of error the number and characteristics of the original 'primary races' of man. Whereas in the evolution of animals, there is a constant branching, each branch being permanently isolated after a certain degree of differentiation by becoming incapable of fertile crossing with other branches, in man the branches constantly meet and unite and produce new types of shoots. The conventional ancestral tree is a suitable method for representing the descent of animal types: it is wholly unsuitable for man. Further, while animal types can be reasonably classified on the basis of using degrees of resemblance to indicate degrees of relationship, no such simple scheme will serve for man. In fact, with a species in which intercrossing of divergent types is so prevalent as our own, *no* simple system of classification can ever be devised to represent the realities of the situation.

Theoretically, we could give a scientific description of human groups by means of the frequency-distribution of different hereditary factors or genes which they contain, and the correlations denoting the tendency for the different genes to be associated. This, however, is at the moment a mere ideal (save for the blood-group genes<sup>1</sup>) and is never likely to be fulfilled except for the genes responsible for a selected group of well-marked physical characters. In the absence of this, we

<sup>1</sup> An article on blood-groups and race, by Dr Millot, will be published in the next number of *ANTIQUITY*.



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are driven back on measurable physical characters: here again we must content ourselves with the frequency-distributions and inter-correlations of various well-marked characters to be found in particular groups. Our picture of the human species will be like a contour map, a region of high frequency for, say, round-headedness being separated from another similar peak by a 'valley' of low frequency; the gradients in frequency will of course vary in different directions. Furthermore, the contour map for one character will not necessarily resemble that for another.

These considerations rob the term *race*, as applied to existing human groups, of most of its significance. All existing groups must have owed a great deal to crossing. It is only when a group has been relatively isolated for long periods that it will, under the influence of selection, have achieved such stability. Such stabilized groups might be called 'secondary races', but they are extremely rare.

In most cases it is impossible to speak of the existing population of any region as belonging to a definite race, since as a result of migration and crossing it includes many types and their various combinations. For existing populations of particular areas, the word *race* should be banished, and the descriptive and non-committal term *ethnic group* should be substituted.

With regard to the separate types which can be distinguished within an ethnic group—for instance in Central Europe today the tall fair-haired Nordic type, the medium-statured, round-headed Alpine type, the short, dark-haired long-headed Mediterranean type—these might perhaps be called 'racial types' and taken to represent originally pure stocks which have later crossed with each other. They would then represent further geographical subdivisions of the original 'primary races' of man. However, such a supposition begs a number of questions. Above all it presupposes that these hypothetical stocks once existed in a state of complete or almost complete genetic purity. There is no concrete evidence for this, and indeed much evidence to show that mixture has been proceeding not merely during the historic period, but back far into prehistoric times. It is more probable that even six or seven thousand years ago, such groups only represented points of high frequency for certain characters on the ethnic contour map, and had already suffered much crossing with other groups. *Race* has so many connotations—of homogeneity, of purity of descent, and so forth—that it is a pity to use it where we are not certain of our ground. Accordingly it is better to resort to a non-committal term like *ethnic*



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*type* or *genetic type* rather than *racial type*. In so far as the different types were once geographically isolated, they may be called 'secondary subspecies'.

A given region of Europe is thus populated by a highly mixed ethnic group in which a number of distinct ethnic types and the products of their intercrossing are to be distinguished. The ethnic types are to be found more or less pure and in relatively high concentration in certain areas—for instance the Nordic type in northern Scandinavia and the Mediterranean type in southern Italy. There is reason to suppose that in earlier times they existed in higher concentration and greater genetic purity, but never wholly undiluted or free from crosses with other types. They would thus represent partial geographical differentiations of the human species, of a kind rather different from anything found in animals. Their later intermixture produces wholly new combinations of characters, which may become stabilized as new 'ethnic types'—a process apparently without parallel in other organisms.

Before proceeding further, there are one or two other popular fallacies waiting to be exploded. One is the misconception that a race is a collection of people all descended from a single original couple. This idea seems to spring largely from the family trees beloved of genealogists of an earlier century, in which a family is traced back to a single founder and his wife. Such family trees in reality trace the descent of a *name*, and have little to do with biological heredity; they are social rather than genetic. On the genetic plane, the idea of descent from a single couple is in any case vitiated for man by the amount of intercrossing which has taken place between groups. Our ancestry will diverge as well as converge as we trace it back. With animal evolution, in tracing back the pedigree of a group you will, it is true, find convergence of separate branches (species, genera, etc.) to a common stem. But this is not the same thing as convergence to a common ancestral pair; and as a matter of fact we can be certain that the common stem is always a large body of individuals, with a certain degree of genetic variability. Change in evolution does not take place through the sudden appearance of a new sport in a single male and female who then become the ancestors of the new species or strain; it occurs by means of the spread of mutant genes through the population. The evolving strain is the whole population of a given area.

Another and more serious misconception is that language is a criterion of race. There are a great many examples in history of a conquering people forcing its language on the conquered; and also a



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great many examples of the converse process, of the conquering invaders adopting the language of the country they have invaded. It is thus quite improper to speak of the 'Celtic race'. There is (or rather was) a group of Celtic-speaking peoples, but the fact of their all speaking Celtic is no proof of common descent or genetic affinity. Similarly there is not and cannot be such a thing as an Aryan race, since the term Aryan refers to language.

This fallacy with regard to language is a particular case of a similar fallacy with regard to social culture in general. Habits, traditions, machines, dress, art, institutions, gestures, ideas—all these as well as language are part of the social environment of human beings. They are not inborn, but have to be learnt or built up by experience. None of these can serve as any criterion of racial affinity between peoples. For instance, if we had nothing to go by but specimens of machinery and other material objects, we might conclude that the modern Japanese were much more closely related to the Europeans than to the Tibetans or other Mongolian peoples: we should, however, be wrong. Culture, both material and spiritual, can spread by culture-contact, whereas physical characters can only spread by actual intercrossing.

Nor can the cultural level of a people serve as evidence of its innate ability or the reverse. The ancient Romans, perfectly correctly, regarded the inhabitants of this country and of Germany as uncivilized. They would have been considerably surprised to find them leading the way in civilization a mere fifty or sixty generations later. In ancient Greece, some very eminent philosophers went further and ascribed to the northern barbarians an innate incapacity to rise to the attainments of the Greeks. They were confusing cultural level with innate capacity. The rapid rise of Arab culture to a high intellectual level and its subsequent fall and stagnation is another example. We must beware of falling into the same fallacy ourselves as regards the so-called 'backward peoples' of the modern world. It is by no means impossible that our attitude of superiority towards the African native, for instance, may prove to be as unjustified as that of the ancient Greeks and Romans to our own ancestors.

On *a priori* grounds we may expect differences in innate ability to exist between different peoples. But achievement by itself is no guide; and so far no satisfactory method has been devised of testing differences in innate intelligence or other psychological qualities between people with very different education and culture. The differences in social environment override the differences in genetic equipment.



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Finally there is the fallacy which equates *race* with *nation*. Here again there is confusion between a genetic and a social concept, but the case demands special treatment because of the special dangers inherent in this particular fallacy. The most obvious way of demonstrating that it is a fallacy is to consider the United States of America. There is, very definitely, an American nation, whose nationalism has indeed been growing more pronounced during the last few decades, but, equally definitely, there is no possibility of speaking of an American race. Indeed the popular phrase so often applied to America, 'the melting-pot of race', demonstrates the contrary. A nation is a group of people with a common tract of country, bound together in a common State by common history, common sentiment and traditions, common social organization, and usually (though not always—witness Belgium or Switzerland) by common language. It is also bound together by being the unit to which the individual belongs, so that he regards other individuals belonging to the same unit as in some way allied with him, while individuals belonging to other similar units are 'foreigners', in some way alien to him. This sense of solidarity with co-nationals and of separateness from other nationals is of the essence of nationality. The nation, however, is a particular phase in the evolution of human groups, not anything permanent or inherent in human nature. It is in essentials a product of the last three hundred years, and quite different in nature and organization from other units of the same general type, such as clan, tribe, city-state, or empire. The idea of the blood-tie has been used to strengthen national sentiment because of the importance of such sentiment for unity and effectiveness in war. But mass migration and military conquest, and the adoption of foreigners into the group by legal change of citizenship and by marriage, when the wife and children follow the father's nationality, make the thesis of common descent impossible to uphold. The idea of a British, a French, a German, or an Italian race is a political fiction, and a dangerous one at that.

Coming down from the general to the particular, we can here deal with two so-called 'race' problems which are of immediate political importance—the Nordic and the Jewish. Beginning with the latter, we find that the Jewish problem is far less a racial or genetic than a cultural one. The Jews are no more a distinct sharply marked race than are the Germans or the English. They are originally of mixed descent, comprising true Semitic, Hittite, Armenoid, and probably other elements. During their dispersal they have interbred, legitimately and illegitimately,



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with the surrounding population so that a number of genes derived from the immigrant Jews are scattered through the general population, and the Jewish communities have come to resemble the local gentiles in many particulars. In this way the Jews of Africa, of eastern Europe, of Spain and Portugal, of Germany and so on, have become markedly different from each other in physical type. What they have preserved and transmitted is not racial qualities, but a religious and social tradition. The Jews do not constitute a definite race, but a pseudo-national group<sup>2</sup> with peculiar historic traditions and a strong religious basis. Biologically, it is as illegitimate and almost as absurd to speak of a Jewish race as of an Aryan race.

The Nordic theory is in another category. Instead of ascribing racial qualities to a group which is today essentially held together on a cultural basis, it takes a hypothetical past race, ascribes to it certain valuable qualities, notably initiative and leadership, and then whenever it finds such qualities in the mixed national groups, ascribes them to the Nordic elements in the population. It then goes further, and sets up as a national ideal a return to purity of Nordic stock. This theory has exerted a considerable influence on the immigration legislation of the United States, and, in combination with a theory of Jewish racial inferiority, is playing a leading part in shaping the course of affairs in Germany today.

The facts of the case are as follows: The Nordic race, like other 'pure races', is hypothetical. There does, however, exist a Nordic type. This occurs with only a moderate degree of mixture in parts of Scandinavia, and is also to be found, but much mixed with other types, so that all intermediates and recombinations occur, in northern Europe from Britain to Russia, with pockets here and there in other countries. On various grounds we can be reasonably sure that this distribution is the result of the invasion of Europe by a group largely composed of men of this type—perhaps in the degree of purity in which the type is now found in parts of Scandinavia. This group was the 'Nordic race'—a secondary subspecies in our sense. It is not certain where it originated, or when its important migration took place, but many authorities believe that it came originally from the steppes of southern Russia.

But besides these facts and deductions, there exists what one can only characterize as a Nordic myth, ascribing to the Nordic race most

<sup>2</sup> The term *pseudo-national* is used for want of a better. It is not intended to imply that they constitute a nation in the accepted sense of the word.



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of the great advances of mankind during recorded history, and asserting that their qualities of leadership fit them to rule over other races. The Aryan and Germanic myths are variants on the same theme.

These contentions appear to be based on nothing more serious than self-interest and wish-fulfilment. In the first place, it is quite certain that the great steps in civilization, when man learned to plough, to write, to build stone houses, to transport his goods in wheeled vehicles, were first taken in the near East, by peoples who by no stretch of imagination could be called Nordic, but who seem in point of fact to have been largely of the dark, Mediterranean type. Secondly, it is true that great advances in civilization have sometimes been observed in history when invaders of relatively light-skinned type have irrupted into countries populated by other groups—notably in Greece, though here round-headed as well as long-headed elements were included among its invaders. But in such cases, both types appear to have made their contribution, and the result can be ascribed to the vivifying effects of mixture and culture-contact with as much propriety as to the inherent qualities of one of the types concerned. Indeed, where the Nordic type is most prevalent, in northern Scandinavia, there is also found among the people a tendency to introspection, accompanied by a very high suicide-rate; this may well be an effect of the northern environment, but may equally well be a characteristic of the type. The greatest achievements of modern civilization have, by and large, occurred in regions of the greatest mixture of types—Italy, France, Britain, and Germany, to mention only four nations. In all these countries of 'mixed race', owing to the nature of Mendelian inheritance, it is rare to find pure Nordic types. The great bulk of the population will contain genes derived from two or more original sources. In a nation like Britain or Germany, the Nordic type is irrecoverable for the country at large; the population as a whole is an inextricable mixture. The Nordic type may be held up as an ideal, but this ideal is genetically unattainable, and will not affect the biological realities of the situation.

Furthermore, when we look into the facts of history, we find it far from established that men of pure or even approximately Nordic type have been the great leaders of thought or action. The great explorers of Britain displayed initiative, but hardly any one of them was physically of Nordic type: the majority of the most celebrated Germans, including Goethe, Beethoven and Kant, were medium- or round-headed, not long-headed like the typical Nordics. Napoleon, Shakespeare, Einstein—a dozen great names spring to mind which in themselves should be



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enough to disperse the Nordic myth. The word *myth* is used advisedly, since it frequently plays a semi-religious rôle, as part of the theology for a creed of passionate racialism.

From what has been said, it will be clear that 'race-mixture' has in the past been beneficial. The British contain strong Nordic and Alpine elements, with a moderate admixture of Mediterranean types. In the Germans, there is a much stronger Alpine element, and genes from the yellow races have crept in *via* Russia. In France, a very similar mixture is visible, with a greater preponderance of more extreme Mediterranean types in the south. The Jews are of mixed origin, and have been steadily growing more mixed. America is proverbially a melting pot. The Japanese are so mixed that no full account of their origins can at present be given.

In human affairs it is usually impossible to say which of a number of possible causes is decisive. A nation with a mixed population achieves great things. Is its greatness due to the genetic mixture in its people, or to the culture-contacts that have been brought about? Or is it due to neither of these things, but to favourable economic circumstances? It is never possible to be sure: it may be due to all three simultaneously. At any rate, we can assert without contradiction that genetic mixture of human types is certainly not harmful, and that it is to be found in every people which has achieved great things in history.

But this does not imply that race mixture must always be good. In the above examples, we are dealing with mixture between minor subdivisions of one 'primary race'. What of mixture between the major subdivisions of mankind—between black and white, for instance, or white and yellow? In this field, the most violent feelings are aroused. We need only cite the strong feeling in the United States against white intermarriage with negroes or Chinese, in South Africa against the Bantu admixture.

When we make a comparative survey, however, we find that this prejudice is not universal. The feeling against intermarriage with black or brown is far less marked in some countries—e.g. most Latin nations—than in others—e.g. Britain or the United States. For instance, Portugal contains a considerable proportion of negro genes, amassed by crosses in Brazil. In the South American countries there is little or no social discrimination against the offspring of mixed marriages, and Indian, negro and white stocks are inextricably blended. The population of Mexico is predominantly one of hybrids between white and Indian. In the British West Indies there is a large and respected class



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of half-castes arising from negro-white admixture. In Soviet Russia, there is deliberate discouragement of all 'race prejudice'.

We need not multiply examples. What seems evident is that in this field too, it is not biological but cultural factors which are dominant. Where for instance a slave-class exists of markedly different ethnic type from their masters, it is clear that marriage with one of the slave type will be frowned upon. This will not, however, prevent the occurrence of sexual relations between the two groups, though these will be almost entirely between the men of the ruling class and the women of the slave class or type. The most obvious example is that of the relations of planters and slaves in the Old South before the Civil War. Far from there always existing a sexual repulsion between markedly different types, there is often a strong sexual attraction. The disapproval of 'miscegenation' is primarily social, not biological.

This does not, of course, mean that biological factors do not exist. They do exist in regard to the emotional attractiveness, or the reverse, of different ethnic types for each other. However, when we are dealing with the major divisions of mankind, such emotional barriers are certainly not of a primary nature. In suitable social environments, blacks, whites and yellows may exert a powerful sexual attraction on each other.

There is also the question of the biological results of wide crosses. Here it is extremely difficult to come to any firm conclusion. Sweeping assertions are often made to the effect that half-castes are always unreliable, that they share the defects of both parent stocks, and so on and so forth. Such statements usually have no biological foundation. In so far as they are based on facts at all, the facts are social. In many countries where diversity of ethnic type exists, half-castes are at a grave social and often at an economic disadvantage. This is usually so when the dominant or ruling caste is of different ethnic type from the bulk of the population, as between white and negro in the United States, or between white and brown in India. The half-caste is looked down upon by the dominant class, while he is often regarded as alien by the native or dominated class. Small wonder that he develops an unsatisfactory mentality.

Dismissing such cases, we are however confronted with the possibility that very wide crosses may give biologically 'disharmonic' results in later generations, by producing ill-assorted combinations of characters. Characters may be unduly exaggerated by the coming together of unfamiliar genes, or characters adapted to one environment may be forced to co-exist with those adapted to another.



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If the primary subspecies of man were really developed in comparative isolation, each adapted to a different main type of environment, it may be argued that to upset the adjustment brought about by millennia of selection is bound to produce some disharmony. Further, if it be true that some ethnic groups possess a low average level of innate intelligence, to allow crosses between them and more intelligent types is a retrograde step.

These objections undoubtedly have some validity. On the other hand, there are arguments on the other side. Even if wide crosses should produce some disharmonic or maladjusted types, this will occur as the result of the great variability induced by such crosses, and this same variability may be expected to throw up also some exceptionally well-endowed types. Again, because types were well adapted in the past does not prove that new types may not be better adapted to the wholly new environments which man is busily creating for himself, and it may well be that new combinations of characters will be needed to cope with the problems of the future.

In general, however, the arguments on both sides suffer from their extremely theoretical nature. The consequences on both sides should logically follow from the premises, but we have no means of evaluating them quantitatively. When we come to facts, it appears that white-Japanese and white-Chinese crosses give very favourable results.

In any case, it would appear that the chief dangers, or at least those which at the moment bulk largest, are here too of a social rather than a biological nature.

Crosses between groups or classes of markedly different ethnic types within the same country, when they differ also in standard of living, social status, or general outlook, will have the effect of blurring the social barriers and economic distinctions between them. In certain cases, the ruling caste or class may find itself or its ideas swamped and diluted to vanishing point; in other cases it feels impelled to guard its privileges. From a purely biological standpoint it might conceivably be a good thing to undertake mass crossing between say the British and the Bantu, or the Americans and the Chinese, on account of the new genetic combinations to be obtained. But the social systems involved would be unable to stand the strain. There is a limit to the amount of foreign stock which can be taken up by a nation in a given time. This is not due to the quantity of foreign genes *per se*, but to the mass of alien habits, prejudices, ideas and resistances which have to be overcome and assimilated.



## THE RACIAL QUESTION—THEORY AND FACT

Once again the term 'race' proves to be misleading. The question of 'race-mixture' turns out not to be *primarily* a matter of 'race' at all, but a matter of nationality, class or social status. 'Racial crossing' may be inadvisable, but chiefly because the ethnic groups involved happen to be in different national worlds or on different cultural levels. Policy in this matter can only be determined on its economic and social merits; the biologist and the eugenicist have here a negligible or at best a minor role to play as advisers. When politicians and propagandists seek to make out a purely biological case against intermarriage with such-and-such a people or ethnic type, we can always be sure that this is a rationalization, and that the real antagonism is to be sought in some other sphere. The biological reasoning is a cloak to fling over obscure, perhaps unconscious feeling. The ethnic group thus discriminated against serves as the scapegoat for some economic fear, some class prejudice, some nationalist ambition, some cultural pride, even some 'inferiority complex'. Similar types of rationalization, with the same lack of biological validity, have been employed to justify slavery, the caste system, class oppression, religious persecution. The argument against so-called miscegenation is only one among many thrown up by groups in power to justify themselves in their own eyes or in the eyes of others.

So far we have been considering only wide crosses, between markedly distinct ethnic types. Still more do our conclusions apply to crosses between minor varieties of the same primary subspecies, such as Nordic and Alpine, or ethnic groups differing primarily in quantitative ways, such as French and German, or sections of the population distinguished mainly on a cultural basis, such as Jew and Gentile, Catholic and Protestant. In such cases, when biological arguments are advanced to the detriment of one of the ethnic groups concerned, whether against intermarriage or civil equality, religious or economic freedom, we can be certain that these conceal another and more real reason, and that this is either fear of economic competition, or the psychological need of discovery of a scapegoat—or sometimes both in combination. We see this in the United States Immigration laws aimed at the populations of south and southeastern Europe, and in the anti-Jewish legislation and propaganda in Nazi Germany.

In Germany today in order to establish 'Aryan blood' a man must present a pedigree clear of 'non-Aryan'—*i.e.* Jewish—elements for several generations back. The enormous number of cases in which one parent or grandparent of the most thoroughly 'German' citizens has



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proved to be Jewish shows how impossible it is to secure a 'pure Nordic stock'. Once more, indeed, the social and cultural plane is the more important. Germany has benefited a great deal from her Jewish elements—we need only think of Heine, Haber, Mendelssohn, Einstein. But in the economic depression, the competition of Jews in the professions, in finance and retail trade, was proving embarrassing, and in the revolution it was convenient to erect the Jews into a collective internal scapegoat, who could be blamed for mistakes, on whom to vent anger that must be restrained against external enemies. The danger, if danger there existed, was that in a time of exaggerated nationalism, the mass of Jewish tradition and culture was too large to be assimilated properly by the nation at large. Jewish germplasm in Germany has already been so much diluted by Nordic and Alpine genes, and has sent so many of its own genes circulating among the general population, that to attempt restrictions now against intercrossing is like locking the stable door after the horse has been stolen. In any case, there are no biological dangers involved, especially as on intercrossing the religious and cultural traditions of the Jews tend naturally to break down, and this in its turn helps to prevent the persistence of any differential reproduction-rate which may exist as between Jew and Christian.

It is instructive to compare the treatment of the Jew in Germany with that of the Kulaks in Russia. The Kulaks, by standing in the way of rural collectivization, were an obstacle to the Government's economic plans; they also provided a convenient scapegoat for any mistakes and failures that might occur. Their persecution was as horrifying as that of the Jews. But at least it was not justified on false grounds of mysticism or pseudo-science. Their existence obstructed something which was of the essence of communist planning, and they had to submit or be killed or expelled. The Jews could not even submit. Because a false Absolute of race had been erected to cloak the economic and psychological motives of the régime; they could only suffer at home, or in some cases go into exile.

Similarly in America. The real danger was that the American tradition might not suffice to absorb the vast body of alien ideas in the brains of the new immigrant hordes pouring into the country, that the national melting-pot might fail to perform its office, and might crack or explode. When immigrants came in small numbers they could be and were absorbed, from whatever part of Europe they chanced to hail, and in one or at most two generations had become an integral part of the American nation. Their Alpine or Mediterranean genes stood in the



## THE RACIAL QUESTION—THEORY AND FACT

way of the process no more than their previous Czech or Italian nationality. It was the size of the blocks of extraneous culture presented by twentieth-century immigration which constituted the problem.

It may well be that in many cases the discouragement of 'racial crossing' may be the correct policy. But this will be primarily because such crossing will be a symptom of large-scale culture-contact, and that this culture-contact is in danger of bringing about economic or cultural changes which are undesirable, either in themselves or as a matter of immediate policy. In such cases it is best to prevent intermarriage (and extra-marital intercourse); but to do so by preventing the large-scale culture-contact—for instance, in inter-tropical Africa, by reserving as much as possible of the country as native areas, to pursue a development based on indigenous tradition and heading towards an African ideal, rather than to aim at introducing a permanent white element in economic competition with the native population.

So long as nationalist ideas, even in modified form, continue to dominate the world scene, the large-scale segregation of areas, each developing their own general type of culture, may be the policy to pursue. If unrestricted immigration seems likely to upset such a policy, restriction is justifiable, as with Asiatic races in Australia, and in the United States. But do not let us in such cases make a fetish of 'race', or become mystical on the subject, or justify ourselves on false biological grounds.

Such considerations, however, apply only to major differences of type and culture. Within a single main ethnic group or cultural area, they are irrelevant. Notably in Europe, both ethnic intercrossing and culture-contacts have proceeded so far that 'racial purity', like complete isolationism or self-sufficiency, is impossible of attainment. And because they are impossible to attain, they are dangerous to aim at; as an ideal, they contain unrealities and impossibilities which may destroy essential realities and thwart true possibilities.

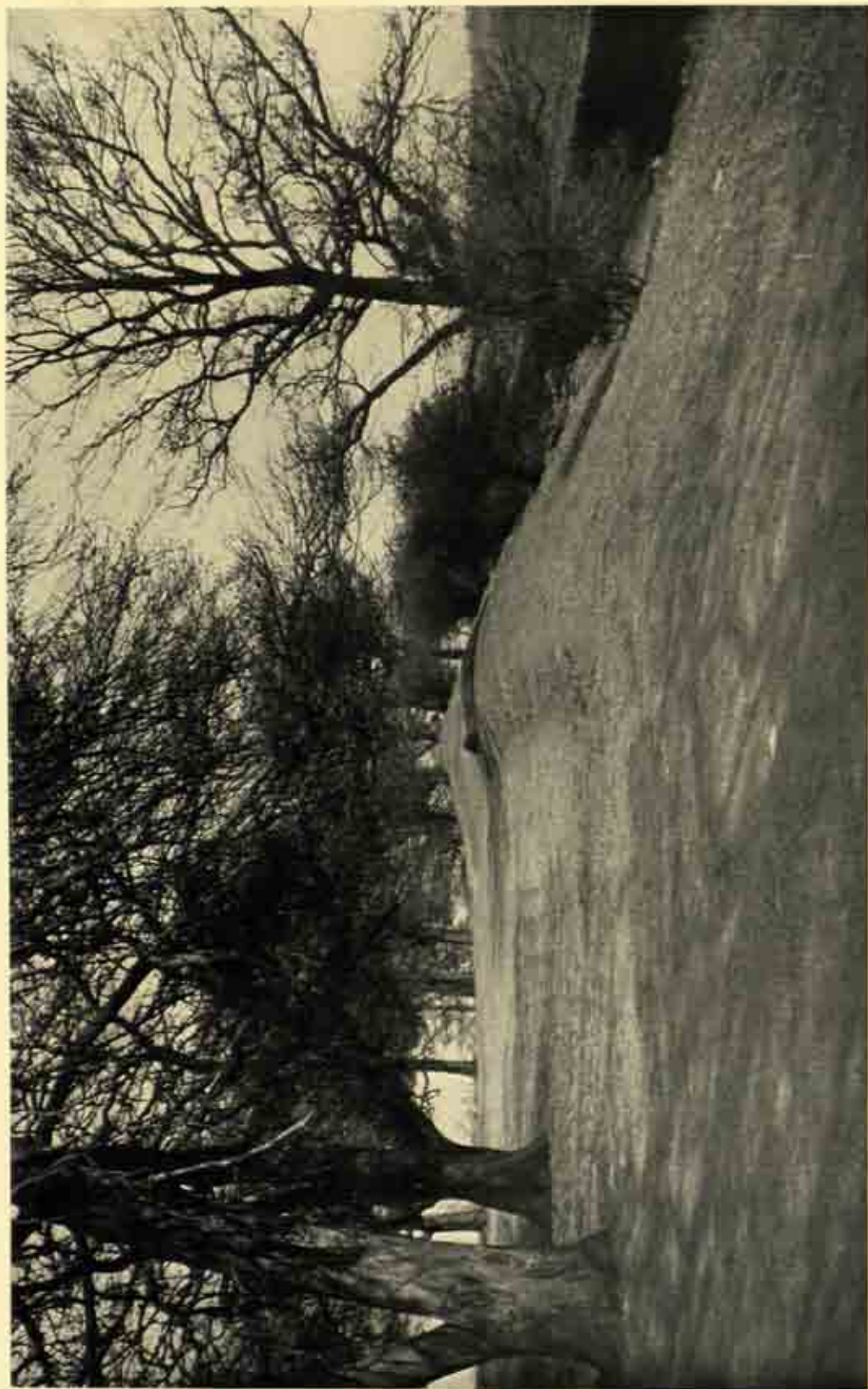
The violent racialism to be found in Europe today is a symptom of Europe's exaggerated nationalism; it is an attempt to justify nationalism on a non-nationalist basis, to find a firm basis in objective science for ideas, passions, and policies which are generated internally by a particular economic and political system, and have real relevance only in reference to that system. The cure for the racial mythology, with its accompanying self-exaltation and persecution of others, which now besets Europe, is a tempering of the nationalist ideal, and in the practical sphere an abandonment of claims by nations to absolute sovereign rights.

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To drive this lesson home will take time, and may even demand a new period of war. Meanwhile, however, science and the scientific spirit can do something by pointing out the biological realities of the ethnic situation, and by refusing to lend her sanction to the absurdities and the horrors perpetrated in her name. Racialism is a myth, and a dangerous myth at that. It is a cloak for economic and nationalist aims that in their uncloaked nakedness would look ugly enough. And it is not scientifically-grounded. The essence of science is the appeal to fact ; and the scientific facts remain.







THE SCOTS DYKE AT OLLIVER PASTURE, NEAR RICHMOND, YORKS (See p. 284)  
/W. O. G. & Crawford



## Arthur and His Battles

by O. G. S. CRAWFORD

THERE are two Arthurs; the Arthur of romance created by Geoffrey of Monmouth in the 12th century and revived, with embellishments, by Lord Tennyson in the 19th; and the Arthur of history. The one has profited greatly by the renown of the other. It is quite certain that no such person as the Arthur of romance ever existed, and he may therefore be dismissed by the historian as a character in fiction. There are some who even doubt the existence of the historical Arthur; but we are bound to accept him for several reasons. He is mentioned by name in the compilation called *Historia Britonum* and the passage in question, which is our only source for the twelve battles, belongs to the oldest portion of the work, and shows the vigorous development of legend about the hero's name within something like one hundred and fifty years of the time when he must have lived.<sup>1</sup> Arthur is referred to by name in some of the Welsh poems which are accepted by scholars as originally composed long before the time of Geoffrey of Monmouth; and he is mentioned alongside of other heroes whose existence has never been questioned.

The strongest argument against the existence of Arthur is the silence of Gildas, a contemporary writer. It is certain that Gildas does not mention Arthur by name, even in the passage where he refers to the battle of Mons Badonicus, the last and most famous of Arthur's battles. Mr A. O. Anderson has suggested that Gildas had Arthur in mind in a cryptic passage (*urse, multorum sessor, aurigaque currus receptaculi urst*), and he justly remarks that, if this contention of his should be admitted, it must prove the historical character of Arthur once for all.<sup>2</sup> But unfortunately Mr Anderson's ingenious suggestion cannot, from the nature of the case, be proved. After all, why should Gildas mention Arthur? Gildas was a publicist, not a historian; had he been alive today his denunciations would have been published in, let us say, *The Observer* rather than in *History* or *ANTIQUITY*. He may have ignored Arthur because he had nothing against him, or because he thought him

<sup>1</sup> J. D. Bruce, *Evolution of Arthurian Romance*, 1923, I, 9.

<sup>2</sup> *Zeitschrift für Celtische Philologie* XVII, 404-6.



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unimportant, or because he was not a king but merely 'dux bellorum'.<sup>3</sup> He was not writing history, and his oversight, if such it was, may be accounted for accordingly.

The silence of Bede is less disturbing. Bede wrote a couple of centuries after the period of Arthur, and he was therefore dependent upon earlier writers for his information. If they were silent on this matter, so was Bede. Moreover, Bede had no access to Celtic sources. The compilers of the Old English Chronicle could not be expected to mention their most doughty antagonist, and they do not. If we turn to our only other trustworthy sources, the Lives of the Saints, we find occasional references to an Arthur; he is plainly a historical personage, but the Lives that mention him are all late, and it is impossible to discover how much of them is derived from earlier sources. One thing, however, is certain; the Arthur of the Lives is quite distinct from Geoffrey's Arthur. He is merely one out of many petty tribal chieftains; seen through priestly eyes he does not appear as a good man, but then few such did. The Arthur of Nennius, or of some later interpolator, was described as 'cruel from his boyhood, a horrible son, a horrible bear, an iron hammer'; and these epithets tally with the stories in the Lives of Cadoc and Gildas. (See under Loomis in Bibliography, p. 291).

There is one other source, the early Welsh poems. One can hardly call them trustworthy, because both composition and manuscripts are very late—post-Conquest and, what is worse, post-Geoffrey. Yet they are, so far as Arthur is concerned, almost or quite free from any Galfridian contamination; and there is general agreement that they contain a very considerable kernel of really ancient and authentic matter. The difficulty is, to disentangle it from the later embellishments. There is the further obstacle that no good translation of them exists, and that much of what passes under that name is a collection of phrases that conveys no sort of coherent meaning to the reader.<sup>4</sup> One has fleeting glimpses of a warrior-chieftain perpetually fighting, though it is not always clear against whom; and of one who was no greater or more important than several others of his kind.

<sup>3</sup> These words probably described some official military post; Germanus was *dux belli* at the Hallelujah battle: Bede, *De Temporum Ratione* (Works, ed. Giles, 1843, v, 319).

<sup>4</sup> This defect is being remedied by the scholarly research of Professor Ifor Williams, who has already extracted several nuggets from the slag-heaps. See p. 323 of this number for a very full review-summary of his book, published, unfortunately for English readers, in Welsh.



## ARTHUR AND HIS BATTLES

Finally, we come back again to Nennius and his catalogue of twelve battles. From what source did he obtain them? Surely it must have been from some annals, long since lost, resembling the so-called Cambrian Annals which are contained in the same manuscript.<sup>5</sup> A northern source is suggested by the reference, in one manuscript, to Wedale, identified with a site in the valley of the Gala Water now called Stow, six miles north of Galashiels [*Wedale est in provincia Lodonesie nunc vero juris episcopi S. Andreae Scotiae, vi milliaria ab occidentali parte ab illo quondam nobili et eximio monasterio de Meilros*]. But this reference does not appear in the other manuscripts; it is an interpolation, and too much reliance must not be placed upon it. It merely proves that, at the time it was written, there was a local association of a certain northern site with Arthur. Similar associations could be cited for Wales and Cornwall. The fact is that at the beginning of the 12th century Arthur had already become a popular hero in the three Celtic regions of Britain. No doubt it was this fact that led Geoffrey of Monmouth to select him as the central figure of his romance. With the flair of the born journalist he sensed the popular feeling and met with the success he deserved. Unfortunately he posed as a historian, and was accepted as such by posterity.

We can detect the beginnings of the process even in Nennius. It is most improbable that his source, whatever it may have been, gave simply a catalogue of *Arthur's* battles. It is far more likely that Nennius selected them from a general list of events, not necessarily all battles; or he may have attributed to Arthur battles recorded (as they often were) without any information about the combatants involved or the result. The battles of this list may have been—probably were—spread out over a long period. In most annals<sup>6</sup> it is unusual for more than one

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<sup>5</sup> Brit. Mus. Harleian 3859, written in the 11th century and composed about the middle of the 10th. See Phillimore, *Y Cymmrodor*, ix.

<sup>6</sup> One of the sources of the *Historia Britonum* may have been composed by Run, son of Urien (*Run mab Urbgen*); this is implied in Nennius, chapter 63. Now Urien was King of Reged, adjacent to and perhaps part of the kingdom of Strathclyde. What is more probable than that there existed, at such centres of learning as Whithorn or Glasgow, some records of a historical nature? However this may be, the mention of Run is a definite fact in support of a northern origin of part of the *Historia Britonum*, for Run's father Urbgen was a historical character who died between 572 and 579, and who was therefore born not long after the traditional date of Arthur's death (537) and probably a little before it. (See Bruce, *Evolution of Arthurian Romance*, 1923, I, 9 (note 19), quoting Thurneysen, *Zeitschrift für deutsche Philologie* xxviii, 83 ff.)



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battle to be recorded in any given year ; indeed we should probably be not far wrong in estimating the period covered by these twelve battles at about twenty years or more, and at twelve years as a probable minimum. If that estimate is correct, their association all with one man is most improbable. The Arthur of legend retreats into the background and becomes a petty chieftain, or at the most *primus inter pares* ; but we are left with a residue, mostly insoluble, of actual history.

Before considering the battles in detail we must understand the political geography of Britain at the beginning of the Dark Ages. The dominant factor is the existence of two zones, highland and lowland (PLATE II).<sup>7</sup> These distinct areas have determined the course of British history and prehistory from the Beaker invasion in the first half of the second millennium B.C. right down to the Industrial Revolution, when the conditions were reversed. In times of invasion the lowlands were rapidly overrun by invaders from across the North Sea and the Channel, while the highlands remained high and dry. In such times the highlands became areas of refuge from the rising tide of conquest ; but eventually, after a period of border warfare, they were infiltrated by the lowland culture. These two zones correspond pretty closely to Haverfield's division of Britain into a civil and a military area.<sup>8</sup>

It would be natural that the people of the highland zone should acquire some sort of cultural and even political coherence when thrown back on themselves by the conquest of the lowlands, reinforced by piratical descents from Ireland. But no permanent political unity was ever achieved. There may have been temporary alliances ; individual leaders may have achieved some sort of temporary control over allied forces, such as the words '*dux bellorum*' seem to imply. Culturally, Christianity formed a bond uniting Strathclyde, Wales and Cornwall. And here we may note that the highland zone seems to have consisted of two parts, the northern half being southwestern Scotland and North Wales, united by the sparsely inhabited intervening coastland, and the southern being South Wales and Cornwall (with Brittany as a kind of colony). Such a division is of course speculative and need not be insisted upon, for in any case we have evidence of actual contacts between Wales and Scotland. Cunedda left his

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<sup>7</sup> Borrowed, by kind permission, from Sir Cyril Fox's *Personality of Britain*, p. 26 (2nd edn., 1934. Cardiff: National Museum of Wales).

<sup>8</sup> See Fox *op. cit.* figs. 13 and 14, from Haverfield and Macdonald's *Roman Occupation of Britain* (Oxford, 1924), figs. 21 and 20.

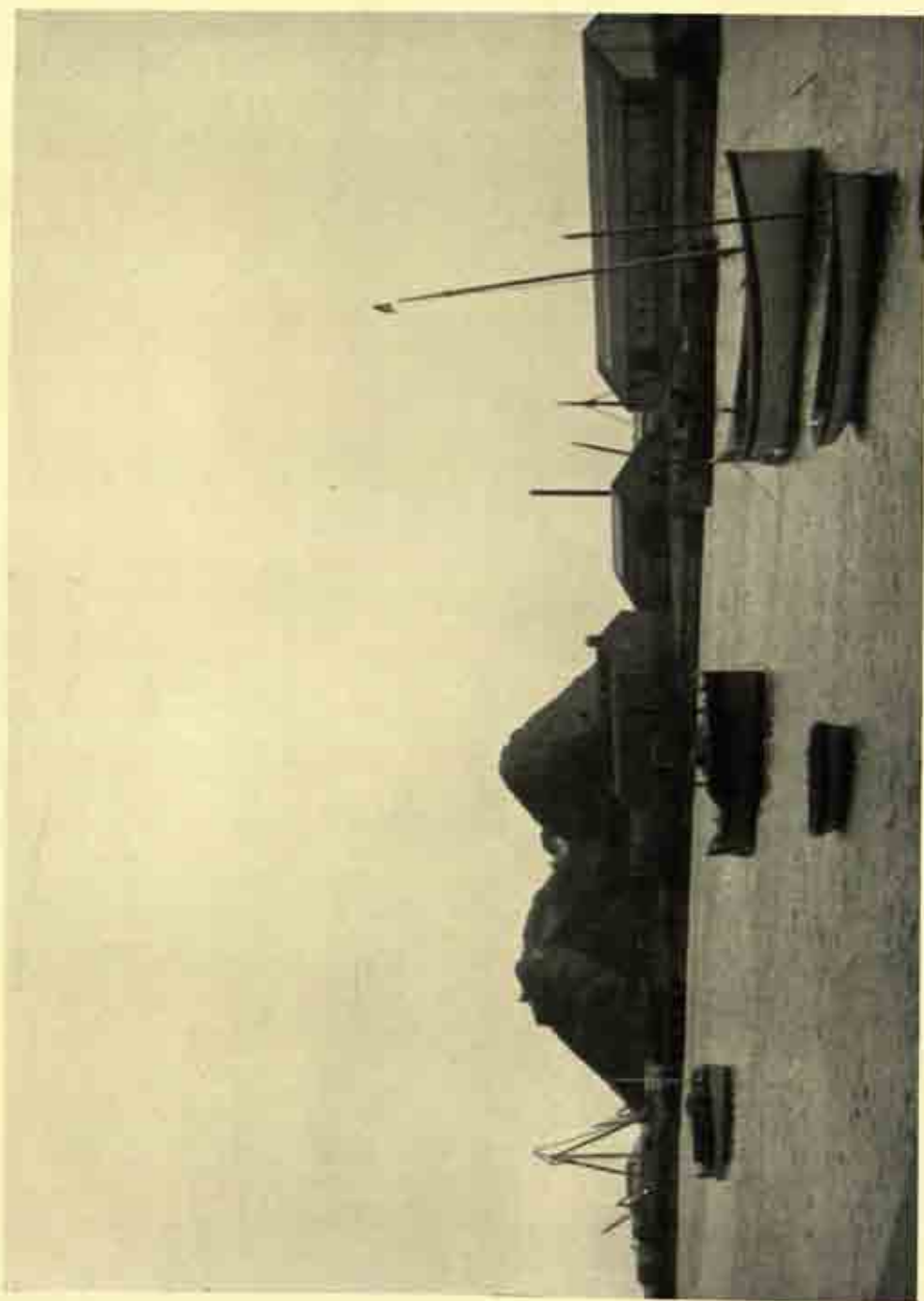


# PLATE II



PHYSICAL MAP OF THE BRITISH ISLES SHOWING, BY DARK SHADING, THE HIGHLAND AND LOWLAND ZONES OF BRITAIN (See note 7, page 286)

PLATE III



THE ROCK OF DUMBARTON (ALTCLUT), STRONGHOLD OF THE KINGDOM OF STRATHCLYDE, LOOKING SOUTH (See p. 284)

Ph. O. G. S. Crawford



## ARTHUR AND HIS BATTLES

kingdom in the Eastern Lowlands of Scotland and settled in North Wales, driving out the Scotti (Irish) about 400. Kentigern followed much the same route nearly two centuries later. Gildas (if we may believe his biographer) spanned the whole tract, having been born on the Clyde (Arecluta), working in Wales and dying in Brittany. Cadoc had Scottish as well as Welsh associations. This unity, such as it was, of the Celtic West, was broken up by two decisive events—the battle of Dyrham in 577, which cut off South Wales from Cornwall, and the battle of Chester in 616 which severed the land-connections between North Wales and Strathclyde. Culturally the results may not have been important, for the sea-ways remained open. But politically it was otherwise; for politics are based upon force, and the slow communications of those days would prevent a concerted attack from the two divided regions upon the common Anglian or Saxon enemy.

The Dark Ages in Britain may be regarded as consisting of a short period of conquest and settlement of the lowlands, lasting perhaps a couple of centuries at the most; and a prolonged period of spasmodic warfare with the Celts of the highland zone which continued until the Norman conquest of Wales, though it ended sooner in the North.\* At the time of the 'twelve battles of Arthur' the land-route between Wales and Scotland was still open and in the hands of the Celts; and although the Angles may have begun to filter into the southern end of the Pennines, in Derbyshire (where barrow-burials of the pagan period are common) it is unlikely that this was a factor of more than local importance. One political centre of Anglian power was in Northumberland, where the earliest known historical kingdom was established later with its 'capital' or tribal stronghold at Bamborough. The whole extent of the eastern slopes of the Pennine ridge, the central watershed of northern England, was a region of friction; and it is here that battles are to be looked for. Particularly north of the Tees; for south of it the habitable limestone uplands of Yorkshire and Lincolnshire were protected by a thick hedge of primeval forest that ran right across England to the mouth of the Severn, breached only by a couple of Roman roads. Indeed the subsequent brilliance of the Northumbrian kingdom may have been due in part to just this fact, that it was *not* geographically isolated. By land it was not a difficult journey up the Tweed into Arecluta and thence to Dumbarton (PLATE III), the capital of the British

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\* A decisive stage of temporary equilibrium is marked by the construction of Offa's Dyke in the 8th century.



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kingdom of Strathclyde.<sup>10</sup> By sea it was easy to sail north, as St. Cuthbert did, to the land of the Southern Picts, or south to the continent. Such maritime intercourse may well have prepared the way for that Anglo-Celtic art which is represented later by the Lindisfarne Gospels (see ANTIQUITY 1934, VIII, 43-57). But all this belongs to later times; what concerns us now is the fact that in the 6th century Northumbrian Angles were brought into direct contact with Celts; and that such contact, geographically inevitable, must have been more frequent and more hostile in the period of the earliest Anglian invasions.

This opposition of a native Celtic highland region to an invading host is not entirely speculative, or merely inferred from geography. It is attested by certain archaeological facts which have not hitherto been observed. The highland zone is defended by a group of six linear earthworks (two of them double), all facing south, southeast, east or northeast, starting from Manchester across Derbyshire to Sheffield and thence by Leeds and Aberford to Richmond and Stanwick. There is no need to describe them in detail since they are marked on the map of Britain in the Dark Ages (South Sheet) about to be published by the Ordnance Survey. They all have relation to Roman roads, and four of the six straddle them. Nico Ditch guards the roads entering Manchester (Mancunium) from the south and east. The Grey Ditch is an outpost looking towards Yorkshire and Lincolnshire. The 'Roman Ridges' between Sheffield and Mexborough delimit the upland frontier with regard to an approach *via* Littleborough (Segelocum, later Tiowulfinga Caestir) and Doncaster (Danum). The other 'Roman' dyke, running north from the Aire at Woodlesford, below Leeds, may have been the frontier of the kingdom of Elmet.<sup>11</sup> The remarkable double lines of the Becca banks bar the passage northwards along what was then and still is the Great North Road.

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<sup>10</sup> It may be conjectured that it was by this route that Lindisfarne communicated with Iona. From Dumbarton the route would be by sea to Lochgilphead, thence by an isthmus road across Kintyre to Loch Crinan passing Dunadd (hence its importance); thence by Eileach an Naoimh (insula Ailech, the site of Brendan Moccu Altí's pre-Columban foundation, testified by existing ruins) direct to Iona across the Firth of Lorn, or possibly to Mull and thence by land.

<sup>11</sup> The exact extent of Elmet cannot now be determined, but Leeds, Barwick and Sherburn were all 'in Elmet' and the last two are still so described. Of these three only Leeds lies behind the dyke. Probably the kingdom and the forest (*silva*) of Elmet were distinct, the forest occupying the lowland area east of the inhabited region, and forming its eastern frontier. The name of the forest would naturally outlive that of the Celtic kingdom, which became merged into the Anglian domains, thus losing its identity.



## ARTHUR AND HIS BATTLES

The well-named Scots Dyke (PLATE I) looks like a distant outwork of the kingdom of Reget; it is thrown across one of the two main roads into Scotland (the western route) at a point just over a mile from Scotch Corner, where the east and west Roman roads to Scotland diverged. It faces east and was set in opposition to any who would cross the watershed by the route leading from Teesdale to the Upper Eden Valley, which historically belongs to the north—a tongue of Scotland protruding towards England.<sup>12</sup> The Scots Dyke touches the fringes of Dark Age history, for only seven miles beyond it was Catraeth, the objective of that famous raid from the land of Gododin, sung by the Welsh bard.<sup>13</sup>

The setting of 'Arthur's Battles' will now perhaps be seen more clearly. It will at least be conceded that a *prima facie* case has been made out for the North in the search for the locality of some at any rate of these twelve battles.<sup>14</sup> Of all twelve, one at least seems plainly to be located in the North, the seventh, 'in silva Calidonis'. It is true that the late Mr W. G. Collingwood attempted to place it in the south of England;<sup>15</sup> but, with the utmost respect for the memory and authority of that great scholar, I cannot accept his thesis. From classical times onwards the Caledonian forest has always been placed in Scotland, and the weight of tradition is to my mind overwhelming.<sup>16</sup> The difficulty has always been to get the Saxons there; but if we adopt the suggestion I have put forward we are not obliged to do this at all. The hypothetical annalist used by Nennius may well have recorded an encounter between, for instance, a king of Strathclyde and a Pictish army. Whether that king was Arthur or someone else we cannot say.\*

Even if this suggestion be turned down, it is possible to provide 'Arthur' with Saxon foes in Scotland; for Nennius himself states

<sup>12</sup> In the 10th century Rerecross on Stanemoor was the southern limit of the diocese of Glasgow and therefore probably of the ancient kingdom of Strathclyde; and one of its limits extended 'usque Loidam civitatem', Leeds. Was the 'Roman Ridge' in Elmet the frontier in this region? See for the evidence, Haddan & Stubbs, *Councils* (1873) II, i, 11; Skene, *Chron. Picts and Scots*, 204.

<sup>13</sup> See Ifor Williams, *Proc. Brit. Acad.* 1932, 270-1.

<sup>14</sup> See also Sir Edward Anwyl's articles in *The Celtic Review*.

<sup>15</sup> *ANTIQUITY*, 1929, III, 295.

<sup>16</sup> A mysterious wood of Celyddon near Arderydd would be a much better region for the battle if it could be authenticated. See Skene, *PSAS*, VI, 94-7.

\* I have already pointed out (*ANTIQUITY*, VIII, 202-4) that *Caer Golud*, associated in a Welsh poem with the name of Arthur, is to be identified with the Northumbrian fort on St. Abb's Head, the *Coludesburh* of the Old English Chronicle.



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that the son and nephew of Hengist (Oetha and Ebissa) were given land 'in those regions which are in the North next to the wall which is called Guaul'. He goes on to state that, having sailed 'around the Picts', whatever that may mean, they ravaged the Orkneys and proceeded to occupy several regions beyond Mare Frenesicum,<sup>17</sup> as far as the frontier (*confinium*) of the Picts. The passage is obscure; we do not know which wall is meant, nor can Mare Fre[n]sicum be identified. Both the Firth of Forth and the Solway Firth have been suggested. The Firth of Forth seems improbable, for it is often mentioned by other writers and this name is not amongst the many applied to it. Moreover the Firth of Forth *was* the frontier of the Picts in the 5th century and for some time afterwards. The words of Nennius do not therefore make sense; it is as if we were to say of the Moors in Spain that they 'occupied several regions beyond the Pyrenees, as far as the frontier of France'. If, on the other hand, it was the Solway Firth, it could mean that they occupied 'several regions' between it and the Firth of Forth. Now there is some slight hint of Frisian settlement on the northern shores of the Solway Firth. The name Dumfries itself *may* mean the Fort of the Frisians;<sup>18</sup> the word Fresicum (if that is the correct reading) should certainly mean Frisian; and the existence of Frisians in this region at a later date is suggested by the character of the names on the Ruthwell cross.<sup>19</sup> Against this is the fact that, so far as we know, the son and nephew of Hengist were not Frisians. It is possible of course that there may have been a Frisian settlement in Dumfriesshire, quite apart from anything that Nennius may have said, though this seems historically improbable.<sup>20</sup>

But we need not be afraid of putting Arthur in the kingdom of Strathclyde; for a Welsh triad gives, as one of his three capitals,<sup>21</sup> Pen Rionyd in the North. The other two occupants of the throne were Cyndeyrn Garthwys, the chief of bishops, and Gwrthmwl Wledic, the

<sup>17</sup> Another reading, palaeographically perhaps less likely, is Fresicum.

<sup>18</sup> Watson, *Celtic Place-names of Scotland*, 1926, 421-2.

<sup>19</sup> Report of the Historical Monuments Commission (Dumfriesshire) 1920, 238.

<sup>20</sup> For the general question see Skene, *PSAS*, IV, 169.

<sup>21</sup> *Lleithiciwyth*, literally 'throne-loads'. For this translation (and for much valuable help in interpreting the above passage) I am indebted to Prof. Ifor Williams, of Bangor University. I should add that Prof. Williams admits the difficulty of understanding exactly what is meant, and that my paraphrase 'capitals' is an inference from the apparent meaning.



## ARTHUR AND HIS BATTLES

chief of elders. Now Cyndeyrn is 'certainly St. Kentigern', according to Professor Ifor Williams, who would prefer therefore to identify Pen Rionyd with the Glasgow foundation of that saint rather than with the Galloway site on Loch Ryan, suggested by Professor Watson.<sup>22</sup> The difficulty is admitted; but in favour of Professor Watson's identification it may be argued that Rionyd and Ryan can be equated philologically: that Ryan is the modern form of Ptolemy's Re-rigionion (meaning 'very royal'), and that close by, though not actually on the shores of the bay is Dunragit, meaning the Fort of Reget. This would be just the place for a royal throne. Whichever be the correct solution, we have an association of Arthur with a British kingdom of southwestern Scotland.<sup>23</sup>

The first of the battles was at the mouth of the river Glein<sup>24</sup> (*in ostium fluminis G.*). At first one thinks that the identification with the Northumberland Glen is obvious and difficult to attack; but unfortunately that river has no mouth! It is the name of a stretch of water between the Bowmont and the Till, and although the word 'ostium' might describe its junction with another river, such a use would be, I think, unusual; unless 'ostium' is a translation of 'aper', as it may well be. In that case the meaning of river-junction is quite normal. The Till formed the boundary of the domains of Lindisfarne in the 12th century (Simeon of Durham, Rolls Series, 1882, I, 199).<sup>25</sup>

The second, third, fourth and fifth battles were on a river called Dubglas 'in regione Linnuis'. Dubglas means 'black water' and is a

<sup>22</sup> *Celtic Place-names*, 34.

<sup>23</sup> As regards the date in question Prof. Ifor Williams writes (in a letter dated 20 Sept. 1933): 'The Triad comes from Hengwrt 536, now Peniarth 45, dated by Dr Evans "late XIII century". Reports I, 379. Triads were collected and written down in the 12th century, *teste* the Black Book of Carmarthen, 1190-1200. The glorification of Arthur in this particular Triad suggests a rather late date, not earlier than the 11th-12th centuries, but before the Geoffrey-Caerleon business. Chronologically, of course, the association of Arthur and Kentigern is not possible'.

<sup>24</sup> The passage in Nennius describing the battles will be found in *ANTIQUITY* 1929, III, 297-8.

<sup>25</sup> *Et hic est Lindisfarnensis terrae terminus: A fluvio Tweoda usque ad Warnamuthe* [MS. Pharnamude], *et inde superius usque ad illum locum ubi haec aqua quae vocatur Warned* [MS. Pharned] *oritur, juxta montem Hybberndune, et ab illo monte usque ad fluvium qui vocatur Bromic, et inde usque ad fluvium qui vocatur Till: et tota terra quae jacet ex utraque parte ipsius fluminis Bromic, usque ad illum locum ubi oritur.* Warnamuthe is presumably in Budle Bay; Hybberndune is Hepburn Moor; Bromic is the Breamish, which becomes the Till at East Lilburn.



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very common Celtic river-name, usually (but not always) describing the brown water derived from peat-moors.<sup>26</sup> There are many rivers once so-called all over Britain, though the modern forms<sup>27</sup> assume many shapes. Without some clue, identification therefore is hopeless. Unfortunately the region of Linnuis cannot be identified with certainty. The suffix -uis (or -wys, in medieval Welsh) represents an older Latin adjectival stem in -ens. Linnuis might therefore stand for Lindenses, just as Cludwys<sup>28</sup> stands for Clotenses, the people of Strathclyde, and Rhegedwys (Recetenses), the men of Rheged. Thus in practice in Welsh the termination always meant 'the people of'. It is, however, a familiar phenomenon that the names of a country and of its inhabitants are interchangeable.<sup>29</sup> 'The old Welsh Linnuis (which would be Llynnwys in medieval Welsh) can come then from Lindensis (provincia) or Lindensia, presumably referring to the Roman town of Lindum Colonia. Lindensia would explain the O.E. Lindissi, the modern region called Lindsey'. But we must not forget the northern Lindisfarne, suggesting another region so called.

The sixth battle was on the river Bassas. No such river is known, and speculative identifications with place-names in Bas- are therefore to be deprecated. The difficulty is increased by the existence of a recorded O.E. personal name Bassa.<sup>30</sup> Mr Jackson suggests that Bassas may contain the word *bas*, shallow, citing Eglwyseu Bassa in the Cynddylan poem, identified by Professor Ifor Williams with Baschurch in Shropshire.

The seventh battle was 'in silva Celidonis, id est, Cat coit Celidon'. This undoubtedly represents 'silva Caledonia' or rather Calidona.\* (The medieval and modern Welsh equivalents are Coed Celyddon). Welsh tradition seems to imply that it was in or near Strathclyde. On the other hand inferences from Latin writers would place it well north of the Forth. Ammianus Marcellinus (A.D. 380) recorded that the Picts were divided into two tribes (gentes), Dicalidonae and Vecturiones; and since the Picts never established themselves south of the Forth

<sup>26</sup> See Ekwall, *English River-names*, 1928, 129-33.

<sup>27</sup> Douglas, Dalch, Dawlish, Devil's brook, Divelish, Dowles brook, Dulas.

<sup>28</sup> Skene, *Four Ancient Books of Wales*, I, 363; II, 162, 406.

<sup>29</sup> I wish to acknowledge gratefully the criticisms of Mr Kenneth Jackson of St. John's College, Cambridge, which have been most helpful in dealing with Linnuis and several other points. The quotation which follows is from a letter from him.

<sup>30</sup> *O.E. Chronicle*, sub anno 669.

\* See Pauly-Wissowa, s.v.



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both these tribes must have occupied land to the north of it. The Dicalidonae are not mentioned elsewhere and the name is puzzling, but it must surely be connected somehow with the common name. Further research is required into the exact connotation of 'Caledonia' in ancient writers.

The eighth battle 'in castello Guinnion' has been attributed to the Roman fort of Vinovia, Binchester, near Bishop Auckland, co. Durham; but it is said that Vinovia would become Gwynwy. The readings in the manuscripts of Nennius are very variable,<sup>31</sup> and one feels that the identification should not yet be entirely rejected. The term 'castellum' would aptly describe a Roman fort, and the situation of Binchester, where the Great North Road of Roman times crossed Weardale, is a likely one for a battle. Weardale was at a much later date one of the principal avenues of Anglian penetration. A Welsh site, 'villam Guinnoui',<sup>32</sup> seems to contain the same word. Dr A. O. Anderson's suggestion<sup>33</sup> of Crunzian in Stow, in the Gala valley, six miles north of Galashiels, seems to require the evidence of early forms to be convincing. The name is written Crunzie on the Ordnance Map, and is the site of an obscure farm of no apparent antiquity or importance.

The ninth battle was 'in urbe Legionis'. This can only be Chester, for the other legionary fortress, Caerleon on Usk, is always so described in Welsh literature, thus implying that when standing alone the 'city of the legion' meant Chester. York, the other legionary fortress, is never thus described. A historical battle at Chester occurred in 616, but, apart from the lateness of the date, this resulted in a crushing defeat of the Celtic side, and the final separation of the Celtic zone into two regions (see p. 280). It is possible that the 'Arthurian' battle may have sprung from an earlier but unsuccessful attempt to drive in this wedge.

The tenth battle was 'in litore fluminis quod vocatur Tribruit'. The manuscript variations are considerable; but they indicate that the proper name consisted of two elements, *tri* and *frut* (oc. \**fruta*).† In some manuscripts the forms Trahtreuroit, Tractheuroit, occur, the first part being the oc. equivalent of the modern Welsh word 'traeth', as

<sup>31</sup> Here too the difficulty of correctly separating the minims is very great.

<sup>32</sup> *Liber Landavensis*, 1893, 179; compare 191 and index.

<sup>33</sup> *Aberdeen University Review*, July 1927, vol. XIV, no. 42.

† Mr Jackson points out, moreover, that there are many philological objections to the explanations I have given here, and on the next page.



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in Traeth Mawr, Traeth Bach, a sandy tract on the sea-shore. The second is a well-known word meaning stream or current; it occurs in Camfrut<sup>24</sup> (oc. \*Cambofruta), Guenfrut,<sup>25</sup> Frut mur,<sup>26</sup> and in the modern names of many English streams.<sup>27</sup> The same place is referred to in the 'Black Book of Carmarthen' (12th century),<sup>28</sup> and it is even possible that one reference (Skene, 1, 262) refers to the 'Arthurian' battle ('Did not Manawyd bring perforated shields from Trywruid?') The site was certainly in the north, in the Gododin region. It is tempting to connect the -frut element with the Fords of Frew, in the Vale of Menteith, and we have Professor Watson's authority for doing so;<sup>29</sup> but earlier forms are lacking. Here the main north and south route of Scotland is constricted between the edge of the Highlands (Ben Venue and Ben Lomond) on the west and the tidal estuary of the Forth at Stirling on the east. The Vale of Menteith itself is covered with peat-moss and before the clearing of Blair Drummond Moss there was only one possible crossing-place, between Doune and Kippen. On the modern road connecting these places are the three farms which preserve the name Frew, though all traces of the fords seem to have vanished. It is significant that one of the four lowland brochs stands on this road at precisely the best point, namely, where it leaves the firm ground on the north. No more suitable place for a fort or a battle could be imagined.

The word 'traeth' is derived from the Latin *tractus*; but it seems always to be applied to a tract of sand on the seashore. Otherwise one would ask whether here it might not have described the not dissimilar inland moor of Menteith; and one might explain the word \*Trifruta as referring to the three streams of Forth, Goodie Water and Teith which traverse it. But all this is highly speculative; one cannot conclude with anything but a regretful statement that the site of the tenth battle is still quite uncertain, though the etymology of its name is plain and an identification seems not so hopeless therefore as it might be.

<sup>24</sup> *Lib. Land.* 1893, 228 (compare 'Lan Camfrout, c'est-à-dire le lann du ruisseau courbe', between Dol and Tref Maoc in Brittany; De la Borderie, *Hist.* 1, 491).

<sup>25</sup> *Lib. Land.* 1893, 222; Ekwall, *River-names*, 1928, 463.

<sup>26</sup> *Lib. Land.* 1893, 122; identified with *ffrwd*, bounds of Llanarth, Mon.

<sup>27</sup> Instances in Ekwall, *English Place-name Society*, vol. 1 (Introductory volume), p. 28; see also Grundy in *Oxfordshire Charters* (Oxf. Record Soc.) p. 27.

<sup>28</sup> Trywruid, Trywruyd, (ar) Traethev Trywruid; Skene, *Four Ancient Books*, 1, 262, 263, 368; 11, 3, 51-2, 321, 351.

<sup>29</sup> Watson, *Celtic Place-names*, 52-3, 349-50.



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The eleventh battle is associated with the name of a mountain called Agned and a battle whose name has so many forms that one despairs even of discovering the correct one, much less of identifying it. Agned is quite unknown; and Skene's identification with Edinburgh<sup>40</sup> proves to have been obtained from Geoffrey of Monmouth<sup>41</sup>—a fact which probably explains why he never gave reasons or references for his identification! With regard to the second name I confine myself to citing the record of a battle in the Annals of Tigernach (s.a. 727) called 'Cath indsi breguin', (the battle of the island of Breguin). It may or may not refer to the same place. De la Borderie's identification with Bravonium (Leintwardine) is obviously impossible.

I do not propose to discuss the battle of Mons Badonicus, as I have no fresh evidence to bring forward.

Arthur was killed at the battle of Camlann, which has come down to us in the garbled form of Camelon. The fact is recorded in Harleian MS. 3859 (composed in the middle of the 10th century) under the year 537. Now Camlann may contain two well-known and quite common Old Celtic words, camb(o)-, curved, and landa,<sup>42</sup> later lanna,<sup>43</sup> an enclosure (represented by the modern Welsh Llan-). There is no place Cambolanda recorded; but Camboglanna ((Birdoswald) comes very near to it, and it is possible to equate Camlann with Camboglanna. The place is a fort on the wall of Hadrian, and was the starting-place of a Roman road running north beyond the wall to Bewcastle and doubtless not terminating there. The name means 'curved glen' and is admirably suited to the topography of Birdoswald.

There is, however, one other possible identification that must be considered. In the lists of the Ravenna Geographer there occurs the name Cambroianna. All three manuscripts give this reading, but it is nevertheless an impossible one, and must be emended. The first and most obvious emendation is to omit the 'r' and write Cambo-. This (as will have been inferred) is quite a common word in Celtic, whereas Cambro- does not occur. Then, the three vowels -oia- cannot occur thus in juxtaposition, and some emendation is demanded. The simplest is to substitute 'l' for 'i'. We then have Cambolanna,

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<sup>40</sup> *Celtic Scotland*, 1876, 1, 153.

<sup>41</sup> Ed. Acton Griscom, 1929, 259 (*agned quod nunc castellum puellarum dicitur*).

<sup>42</sup> E.g. Vindolanda (Chesterholm) the white enclosure.

<sup>43</sup> E.g. Lanna Iltuti, in the Life of Sampson, and frequently in the *Lib. Land*.



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an intelligible word and one which exactly corresponds to Camlann. Unfortunately we don't know where it was; the name occurs in a list of places between the two walls, and in a context which suggests Selkirkshire. One would like to connect it with the fort of Camelon just north of the Antonine Wall, near Falkirk; but this is rather far north for the context, and the few early forms of Camelon are rather against this identification.

In either case Camlann was in the north. There are no other competitors; for the name has nothing to do with the Camels of Somerset or Camelford in Cornwall.<sup>44</sup>

To sum up:—It is reasonably certain that a petty chieftain named Arthur did exist, probably in South Wales. It is possible that he may have held some military command uniting the tribal forces of the Celtic or highland zone or part of it against raiders and invaders (not all of them necessarily Teutonic). It is also possible that he may have engaged in all or some of the battles attributed to him; on the other hand this attribution may belong to a later date. It is the present writer's opinion that the battles were obtained by Nennius from a written source, perhaps in the form of annals, though a bardic source is also possible. The battles may represent the opposition of the inhabitants of the Celtic zone (equated with Fox's Highland Zone) to attempts at penetration by Angles, Saxons and Frisians; but some of them may also represent battles between Celts and Scots or Picts or between Celts and Celts, or between temporary alliances of almost any groups. These struggles formed the theme of bards for ages afterwards, just as did the Trojan war; since they were largely carried on by the Celts for national and cultural independence they were sung in four out of the five large divisions of the Celtic-speaking world, and identifications

<sup>44</sup> I add some references to Camlann:—Bruce (*Arthurian Legend*, 1, 45) quotes Ancurin Owen's *Ancient Laws and Institutes of Wales* (London, 1841), 1, 678: 'When the queen shall wish a song in her chamber, the bard will sing the song about Camlan'. This is from the code of Gwent, the earliest MS. of which, according to Bruce, dates 'from shortly before 1200; but Howel Dda, who is supposed to have compiled it in its original form, ruled in South Wales in the first half of the tenth century'. Stuart Glennie, *Arthurian Localities*, p. Lxi, argues for Camelon near Falkirk, and so do Skene (*Four Ancient Books*, 1, 60, 311), and Dickinson (*King Arthur in Cornwall*, 82). Bruce considers it must have been in the south (1, 73) but for historical reasons now rather obsolete. A 'bellum Camblani' is mentioned in *Vit. Merlini*, line 929 (Faral, *La Légende Arthurienne*, II, 302, III, 334). Gamlan is mentioned in Skene's *Chron. Picts and Scots* p. 161, in a document which he dates 1211. Finally F. Lot has contributed an article (which I have not read) to *Romania*, 1901, xxx, 16 ff, on 'La bataille de Camlan'.



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were naturally and easily made in each region. Such identifications are all late and of little if any historical value.

Finally, the renewed attacks upon Celtic independence which were made between 800 and 1200 may have revived the memory of Arthur in Celtic lands and caused it to be embellished with much imaginative detail. Geoffrey of Monmouth selected Arthur as the subject of his romance, which has no historical value; and though the Arthur there represented is the Arthur of modern popular fancy, he has not the remotest resemblance to the real Arthur of history.

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\* This list is of course not complete. It merely includes some references that ought to be consulted but which are not referred to specifically in the text. I have not been able to consult them all myself.

# Pyramids and their Purpose

## III. PYRAMID MYSTICISM AND MYSTIFICATION

by NOEL F. WHEELER

**A**FTER a consideration of the Pyramid of Khufu and of its original purpose, it will be instructive to take a glance at the vast hosts of the Pyramid Mystics, those who in one way or another extract prophecies from the various measurements of the same Pyramid and its passages. The literature on this subject is almost overwhelming and its value can safely be said to be in inverse ratio to its volume; but there are many whose knowledge of archaeology or of the Pyramid in particular is not sufficient to arm them against this pseudo-scientific obsession.

There have been in the past, and are still, many strange beliefs and cults current in the world, such as flat-earth-ism, the transmutation of Shakespeare into pork, and those excellent people who have bought land on the Mount of Olives in preparation for the Second Coming. They all have their followers, who naturally believe in them, and they are all impervious to common sense. But the Pyramid has attracted a much more formidable concourse than any other subject, largely because of the imaginary mystery of the monument, the remoteness of its date, and the learned formulae and pages of calculus (above the heads of at least 90 per cent. of readers—fortunately), which cloak the looseness of statements of fact by writers on the subject. Let us see where it all began.

From quite early times the fact that the Pyramid was oriented pretty closely to the cardinal points was noticed, as was the roughly pole-star direction of the Descending Passage orifice, and these facts probably led some to think of finding further mathematical peculiarities. Herschel, the astronomer, wrote much in 1860 on these lines in the *Athenaeum*, and John Taylor, the father of the Pyramid mystics, wrote similarly (in 1859) under the title 'The Great Pyramid, why was it built and who built it'? But the greatest fillip was given to the movement by Prof. C. Piazzi Smyth, then Astronomer Royal for Scotland, who at Taylor's request made a scientific examination of the pyramid during the winter of 1864-5. He wrote 'Life and Work at the



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Great Pyramid', in which he started the 'pyramid inch' myth and many others; and everything which has appeared in print on the subject since is founded on Piazzzi Smyth's deductions.

Sir Flinders Petrie came out in 1881 and made his survey of the Pyramid, and his actual figures agree for the most part with those of Smyth, but he gave it as his opinion that there was nothing in the theories. John and Morton Edgar in 1909 were on the track of theories; they measured the Pyramid in great detail and made a number of valuable plans, but went well astray in 'Great Pyramid Passages, etc.', which in its later dress is 'The Great Pyramid, its spiritual symbolism' (1924). In this latter publication are many misstatements of fact, many false deductions, false calculations, and wonderful theories. There are hosts beyond number of minor publications, either specifically on the Great Pyramid, or bringing it and its 'prophecies' in; and there is no point in noticing them since they do but repeat what Piazzzi Smyth, Edgar, and Davidson have said more cleverly. Davidson's book, 'The Great Pyramid, its Divine Message' (1927), is the precocious child of all this family and contains many hair-raising formulae and geometrical figures, all founded on the same sand as the rest.

That this farrago of nonsense should be read at all is remarkable, but that it should be believed, as it is, by large numbers of otherwise normal people is in itself a miracle. Let us take some of the points from the works of Edgar and of Davidson and see what they amount to.

### DIMENSIONS, PROPORTIONS, ETC.

(A) The value of  $\pi$  (3.14159). It is claimed that the Egyptians of the Pyramid Age knew and used this proportion in many places in the Pyramid of Khufu, that the base-perimeter to height proportion is double this.

The base-perimeter to height proportion, as can be seen from the table of ell measures in a previous article, is double  $22/7$  (3.14286); and the Rhind papyrus gives the estimation of  $\pi$  for the Middle Kingdom as 3.16049. So that a knowledge, in the Pyramid Age, of the true value of the  $22/7$  approximation is unlikely. Incidentally, the pyramids of Seneferu, and Henutsen have the same proportion; while those of Khafre, Khafre's Queen, Menkaure, Sahure, Nefer-ir-ka-re and Ne-user-re differ but slightly from it.

In the maze of the  $\pi$  theories we find a statement by Edgar that the sum of the length and breadth of the sarcophagus equals the height multiplied by  $\pi$ . It does not.

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(B) Mathematical miracles. Edgar states that the area of a certain triangle is exactly 20 times the horizontal length of the Grand Gallery. How 20 times a length can equal an area we do not know. Also Edgar

'The fact that there are 5 masonry courses in the King's Chamber walls, and that the topmost contains 7 stones, suggests the number 5.7. . . .'

This requires no comment, and the following is similar :—

'The total weight of every substance in the earth is, therefore, the mean weight of every substance. This mean, or average, weight is the standard for reference. Each individual substance, taken by itself, has a weight which is proportionate to the mean weight. This proportion is known by the term "Specific Gravity" (1).

(C) Levels. Much is made by Edgar and Davidson of levels throughout the Pyramid, sometimes given to hundredths of an inch. An idea of the accuracy to be expected from the builders is got from a series of levels taken along the north face of the platform by the present writer in the course of the survey of the royal cemeteries. Levels were taken at intervals of  $\frac{1}{2}$  base, i.e. at 9 equidistant points including the NE and NW corners (points 1 and 9 respectively). The results were interesting and gave the following differences from the mean value :—

Point 1	— 0.002m	4	— 0.007m	7	— 0.007m
2	+ 0.021m	5	0.000	8	— 0.020m
3	+ 0.006m	6	+ 0.009m	9	+ 0.005m

From this it appears that the builders started with the centre of the Pyramid side levelled, worked from this to the NE and NW corners, then from these corners to the mid-points between them and the centre, and finally from each of the 5 points above to the adjacent  $\frac{1}{2}$ -length point outwards from the centre. The errors have a maximum value of about 2 cms., and an average of 1 cm.; it is foolishness to calculate levels based on these to greater accuracy.

(D) Proportions. Much is made also of the proportions of the Great Pyramid and its chambers, the sarcophagus, etc. That these are what were common in the Pyramid Age can be seen from the following table of the proportions of length, breadth and height, for a length of 1 :—

STRUCTURES		Length	Breadth	Height
Pyramid itself		1	1	.641
4th Dyn. mastaba	IIIa (Junker)	1	.481	.167
	IIIb "	1	.432	.182
	IVa "	1	.432	.182
	IVb "	1	.4	.182
	Va "	1	.404	.17
	Vb "	1	.432	.182
	VIn "	1	.417	.167
	VIa "	1	.455	.182



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CHAMBERS	Length	Breadth	Height
King's Chamber .. .. .	1	.5	.56
Queen's Chamber (full height) ..	1	.911	1.073
(side height) ..	1	.911	.81
Average of 13 burial chambers in 4th Dyn. mastabas (Junker) ..	1	.986	.796
Main hall of Uah-ka's tomb at Qau (Middle Kingdom) ..	1	.598	.379 & .319
Burial chambers in above tomb ..	1	.936	? (two chambers)
	1	.873	? (of the many)

## SARCOPHAGI

Khufu .. .. .	1	.43	.46 (excl. lid)
Khafre .. .. .	1	.534	.447 (incl. lid)
Hetep-heres (Khufu's mother) ..	1	.476	.447 ( " )
Meresankh II (Khufu's dtr.) ..	1	.426	.38 (excl. lid)
Hor-dedef (Khufu's son) ..	1	?	.34 ( " )
Fefi, 4th Dyn. (S. Hasan's 'Giza') ..	1	.454	.391 ( " )
From VIII (Junker) 4th Dyn. ..	1	.394	.34 (incl. lid)
	1	.394	.32 (excl. lid)
From D100 (Junker) 4th Dyn. ..	1	?	.287 ( " )
	1	?	.335 ( " )
Cairo Museum, no. 27, 4th Dyn. ..	1	.517	.63 ( " )
29, " ..	1	?	.598 ( " )
Cairo Museum, no. 31, 4th Dyn. ..	1	.45	.522 ( " )
33, " ..	1	?	.571 ( " )
11th Dyn. Sarcophagus of Sebekhetep at Qau .. .. .	1	.392	.392 (excl. lid)
	1	.392	.498 (incl. lid)
11th Dyn. Sarcophagus of Wah-ka I at Qau .. .. .	1	.4	.398 (excl. lid)
11th Dyn. Sarcophagus of Wah-ka II at Qau .. .. .	1	.394	?
Late Sarcophagus of Pabasa (600 B.C.)	1	?	.288

## ANGLES

Elevation of Ascending Passage, Khufu	26½°	
Descending Passage, 3rd Pyr. ..	26°	
" " Khufu Queen	26½°	
" " 3rd Pyr.		
" " Khufu Queen	27°	
Plug Passage, Wah-ka II, Qau ..	32½°	(11th Dyn.)
Descending Passage in Qau tombs	30°	{ " " }
	32°	{ " " }
	32½°	{ " " }
	35°	

## VARIOUS

Length of one plug-block, Khufu ..	2 ells, 6 hands, 1 finger
" " Wah-ka II (Qau, 11th Dyn.) ..	2 ells, 4 hands, 2 fingers

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From the table (pp. 294-5) it is clear that proportions are very much the same in a number of cases, even from different periods. Compare particularly : (a) King's Chamber and Cairo Museum sarcophagi of 4th Dyn. ; (b) Queen's Chamber and Junker's mastaba burial chambers of 4th Dyn. ; (c) sarcophagi of Khufu, his son Khafre, his mother Hetep-heres, his daughter Meresankh II and others ; (d) passage angles ; and (e) plug-block lengths.

(E) Unit of measurement. The invention of the 'Pyramid Inch', .999 inches, by Piazzzi Smyth was a masterpiece. It was assumed by him to be the unit of measure simply because its assumption made certain measurements fit in with astronomical and other numbers ! The method of making these figures fit exactly is also a masterpiece. If the Egyptians made a passage, as they did, slightly wider in one place than another—inevitable from the nature of their tools and instruments—then a measure between these limits is assumed to suit what is required ; for instance, if the limits happen to be 3 inches and  $3\frac{1}{2}$  inches, then the assumed 'intended' measurement would be taken as 3.14159 and much marvel evoked because it equals the value of  $\pi$ . Another popular method is to add the required amount to a measurement to bring it to what is wanted, and call this addition a 'special number' of the Pyramid or of the chamber in question ! In this way, in various parts of his book, Edgar has 10, 8, 7, 5, 3, 36 as 'special numbers' as convenient ; and multiplies gaily by 100, 1000, and larger numbers as though it signified nothing.

## GEOGRAPHICAL AND ORIENTATION

Edgar states :—

'... there is more land surface in both its (the Pyramid's) meridian and its latitude than in any other meridian and latitude ; while its nether meridian ... ranges its whole length through water except for a short distance near Behring's frozen straits. For this reason, Professor C. Piazzzi Smyth claimed that the meridian of the Great Pyramid is by far the most suitable zero of longitude for all nations'.

The meridians of  $22^\circ$ ,  $28^\circ$  and  $105^\circ$  east, and the latitude of  $45^\circ$  north, all have a longer reach over land, and there are probably many others. The 'short distance near Behring's frozen straits' is merely a distance of some 800 miles through the middle of Alaska !

Another point made much of by Edgar, and repeated by Davidson, is that the true bearing of Bethlehem from the Pyramid is the complement of  $26^\circ 18' 10''$ , which latter is the assumed 'intended' angle of slope of the Pyramid passages between the figures for the actual slopes



## PYRAMIDS AND THEIR PURPOSE

of different passages. Edgar calls this the course by Mercator sailing. Why the modern invention of Mercator's projection should be used for a land journey is not clear, especially as Edgar also calls this the shortest distance, which it is not,—and a straight line, which it also is not. If he wanted the shortest distance he should have used the Great Circle course, which would have given him quite a different distance and quite a different 'course', in fact the latter would have changed progressively throughout the trip. If this bearing had not 'hit off' Bethlehem, one can imagine that it might have met some equally significant site, since that part of the world is full of them. Incidentally Edgar talks of the 'parallel of longitude' of the city of Bethlehem: it must be the only spot that has one. He also refers, in another place, to the capacity of the King's Chamber being so many Pyramid inches, but gets nearer in the next sentence by calling them 'cubical inches'.

On the subject of Orientation Edgar considers that

'no builder could ever orient the sides of a monument with the aid of the magnetic compass; for the magnetic north is many degrees away from the true north. To accurately lay the four sides of the Great Pyramid due north, south, east and west, as they were originally, necessitated either the knowledge of exact scientific astronomy, or the knowledge that can be communicated by Divine Inspiration'.

For the first, if the ancient builder is meant, then he had no magnetic compass anyway; if the modern builder—he can surely calculate the variation. For the second, the variation at present in Egypt is very small, certainly not 'many degrees'. For the third point, it was only necessary to know the Pole Star, which the Egyptians did know quite well. We have excavated and surveyed some hundreds of Old Kingdom mastaba tombs at Giza, the majority of which are oriented similarly to the Great Pyramid; also tombs and buildings elsewhere with the same direction. A Middle Kingdom palace in the Sudan was found to be oriented as exactly north, south, east and west, as any but astronomical instruments could show; and for accuracy of plan this last had half the error per cent. in its diagonals which the Great Pyramid has in its base sides! It was the Egyptian practice to orient their tombs and temples thus as a general rule, though the lie of the land and other considerations caused them to modify it in many cases: frequently 'up-river' or 'down-river' was used as north and south irrespective of the direction in which the stream lay. The object of interest in all these examples belonging to the Pyramid Age was not the north but the east, towards which the temple or chapel faced.



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### CONSTRUCTION

Writing of the emplacement for the bridge-slab in the Grand Gallery, Edgar says :—

' Some think that originally there was no break in the continuity of the Grand Gallery floor, and that thus the entrance into the Queen's Chamber was concealed. But it is more probable that the gap was constructed in order to give the appearance of having been forced. There are certain features which indicate this '.

There are no such features, and the purpose of the gap is obvious at first glance. He also writes :—

' In none of the passages and chambers of the Great Pyramid have we found any of the sculpture-work and carved hieroglyphics which are so common in many of the smaller pyramids . . . '

There is not a single 4th Dynasty pyramid which is inscribed : the idea of the Pyramid Texts did not begin till the 5th Dynasty.

Herodotus actually states that ' on the Pyramid is shown an inscription, in Egyptian characters, how much was expended in radishes, onions and garlic, for the workmen ; which the interpreter, as I well remember, reading the inscription, told me amounted to 1600 talents of silver '. He had obviously been into either the funerary temple of the Pyramid, or more probably into one of the many mastaba chapels. And this is the man quoted as an authority on Egyptian matters by the ignorant ! Herodotus lived longer after the Pyramid Age than we are today after him.

Davidson found material for many pages of theories in an assumption that the ' hollowed in ' core masonry, by which each Pyramid-face of core is ' concave ' to the extent of some three feet in the centre, was repeated in the outer casing. This is known not to be the case. The hollowing-in of the core was most probably a similar measure to the inward sloping bed cut in the rock for the lowest course of casing found in a number of small 4th Dynasty pyramids—intended to give rigidity and stability to the structure. The overlapping of the Grand Gallery courses, the sextuplication of the King's Chamber roof, the massive blocks over the Pyramid entrance, etc., are all examples of the same kind of thing. The same writer, as an engineer, has the following remarks about the plug-blocks :—

' Any engineer, architect, or constructional operative knows that it is impossible to slide or push a block of stone, however smoothly dressed and accurately squared, along a passage after the passage has been completely constructed, if the block fits the passage tightly. It is a matter of experience, in such circumstances, that the block will jamb in the passage unless it has at least  $\frac{3}{4}$  of an inch of clearance all round '.



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It is equally a matter of experience, constructional operatives notwithstanding, that Egypt is full of examples of the Egyptians having done just this very thing.

Edgar states that the sarcophagus must have been put into the King's Chamber before the roof was on, because the entrance to the chamber is too small to admit the sarcophagus. The height of the latter, according to Edgar, is 41.27 inches (max.); and the width 38.72 inches (max.); the height of the entrance passage is 42.5 inches and the width 43 inches, according to Davidson's plans. This is almost as bad as the builders of the tomb of Meresankh, grand-daughter of Khufu, who cut the doorway in the rock wide so as to admit the sarcophagus and then built it in to the normal width with masonry; the joke being that by turning the sarcophagus on its side we removed it through the masonry doorway easily. It was quite usual, on the other hand, for the Egyptians to put in a sarcophagus larger than the doorway before the masonry was closed in, and in the Pyramid they must have done this at least before the Grand Gallery roof was closed, since the sarcophagus could not have negotiated the turn from the descending to the ascending passage.

Davidson says that it is not possible to reach the north-south central plane of the Pyramid except in the King's Chamber, when in fact one can also do so in the rock-cut chamber. On such wild statements are the wilder theories of these writers founded. Davidson also discourses much on the subject of Stonehenge, finding many wonderful things in its measurements; but he states that the area of the circle which is 'precisely internal' to the outer ring of stones is 'exactly' equal to  $\frac{1}{4}$  of the ancient Egyptian aroura. The aroura contains from 29555 to 29427 square feet, according to whether one uses the cubit value of 20.63 inches or 20.59 inches; and Davidson gives the Stonehenge area as  $\frac{1}{4}$  of 29514.3 square feet. However, when we get the area of the Stonehenge circle from the Office of Works plan in *ANTIQUITY* we find that it is  $\frac{1}{4}$  of 30329.9 square feet, which is not quite the same thing.\* The diameter of the Stonehenge circle on the Survey plan is 1179 inches, and according to Davidson's plan 1163 inches, which is not 'exactly' the same.

### ARCHAEOLOGICAL

Edgar tells us that Cheops was named Khufu by modern archaeologists. This should read that Khufu was called Cheops by the Greeks,

\* See *ANTIQUITY*, v. I, 39, 42; II, 235; IV, 143, 340.



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who knew no better. There is some excuse for confusion in names, since eminent archaeologists still persist in many versions of every ancient Egyptian name (see Reisner's on the 3rd Pyramid in *Mycerinus*). Mykerinos was as near as the ancient Greeks could get to Menkauré, but his name was Menkauré for all that.

Edgar also, after telling us that the Pyramid of Khufu was not intended as a tomb and that Khufu was really buried in what is well known to be a tomb of two or three thousand years later date, writes :—

'Cheops . . . therefore, did not intend the Great Pyramid to serve as a tomb ; nor, indeed, if we are to believe the reasonable deductions which are based upon historical accounts, did he or his Egyptian subjects know what purpose this immense edifice was intended to serve ' !

We can picture Khufu and his officials meeting with furrowed brows, and the King saying to them 'What on earth am I building this thing for ' ?

Davidson insists on calling the name of the Pyramid of Khufu 'Khuti'—'The Lights'. The name is 'Khufu Akhet', meaning 'The (eastern) horizon of Khufu', the significance of which will be found by referring to the Pyramid Texts. The name might less probably mean 'Khufu is glorious' or 'The Glory of Khufu', but in any case the name of Khufu is inseparable from 'Akhet' in the name of his Pyramid. The 2nd Pyramid was 'Khafre Wer', 'Great is Khafre'; and the 3rd Pyramid similarly 'Menkauré Netery', 'Menkauré is Divine'.

One thing found continually in works like these (Edgar and Davidson), is that obsolete or unacknowledged authorities are very frequently quoted. Fallacies are thus introduced to the readers, apparently with authoritative backing, but actually with none worth speaking of.

Davidson, for instance, quoting Mr Marsham Adams' 'The Book of the Master', claims to find references to the Great Pyramid in the Egyptian 'Book of the Dead'. He speaks of the 'final chapter' referring to the King's Chamber, but if he means chapter 190 or any of those immediately preceding it there is no such reference; in quoting from this 'final chapter' he is quoting from chapter 161, in which one finds nothing which by the wildest stretch of the imagination could refer to the Pyramid. Incidentally the oldest versions of these papyri are of the New Kingdom, and the only version of the origins of the 'Book of the Dead' which can be quoted with regard to the Pyramid are the Pyramid Texts of the 5th Dynasty. The Pyramid



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Texts are quoted by Davidson in one place thus :—' Unas standeth up and is Horus ; Unas sitteth down and is Set ', when the text actually has ' Horus says " Stand up, Unas " ; Set says " Sit down, Unas "' (Pyr. 473). The authority given by Davidson for this and many other archaeological and philological questions is not one that would be accepted as of much value today.

In the matter of dates, Edgar arrives at 2 B.C. for the birth of Jesus ; while Davidson gives 4 B.C.

### MISCELLANEOUS

Edgar writes of the ' shining white ' Pyramid ; and Davidson goes on to work out a marvellous theory that the sun's reflections and shadows from the Pyramid of Khufu were intended to mark the seasons ! This is accompanied by numerous plates and diagrams.

Granted first of all that the Egyptians did not know their own seasons well enough without a heliograph message to tell them when to plough, sow and reap, one is still up against the question of how it was done. The fine white limestone of Turrâh, which was used for the Pyramid casing—as for the casings of all the best of the Giza mastaba tombs—does not take a fine enough polish to reflect anything very thrilling. It is only nearly white when freshly quarried, and rapidly tones down to a sand colour when exposed to air and sun. Granite or alabaster would have been much more suitable materials for a mirror. We are given as reasons for this the accuracy of the working and jointing of the casing, the fact that flaws on the stone were ' patched ' carefully with limestone insets, and the Pyramid's name ' The Lights '. The concavity of the casing face is also mentioned in this connexion.

The accuracy of the working is equally good in those parts of the Pyramid which could not reflect the sun—the internal passage-system for instance—and the same accuracy is found in other contemporary tombs, such as the casing of Khufu's 2nd Queen's Pyramid. The ' patching ' of flaws was a usual Egyptian practice and is found in most of the Giza mastaba tombs and pyramids ; the Pyramid's name was not ' the Lights ', as explained above ; the casing-face was not concave, as also previously explained.

There is a good deal in these publications about the Pole Star direction of the Descending Passage, and we have an enlightening statement on the subject in ' What saith the Scripture ' ? by Discipulus —

' . . . by the direction in which the entrance passage points to the heavens, as only in this year 2170 B.C. did the Pole Star shine exactly down the passage '.

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followed on the next page by:—

' Anyone accustomed to use a theodolite for taking angles knows how small an accidental displacement may seriously affect the results. This is particularly the case with measurements respecting the apparent position in the heavens of the Pole Star, its movements being so slight, that a thousand years make no very appreciable difference, and its change in even five centuries is probably not measurable '.

And yet these people arrive at the precise date of 2170 B.C. by the Pole Star alignment, and scorn the archaeologists' date of 3500 to 3000 ! Incidentally the field of view from the bottom of the Descending Passage includes practically *one degree* of the sky, vertically and horizontally.

Davidson writes on freemasonry from his ' technical knowledge of the symbols displayed by the Fraternity ', and professes to find their ' repeated application in the design of the Great Pyramid ' and suspects from this ' that the elements of the Order were formulated at a time when all knowledge of that Truth, subsequently to be enshrined in the structure of the Great Pyramid, had not yet been lost '. We know that two symbols used by the U.S.S.R. today are the hammer and sickle ; we also know that both occur in the hieroglyphics ; but we do not therefore infer that Bolshevism was known in the Pyramid Age. Speaking also from our ' technical knowledge of the symbols displayed ', etc., we can say that where objects used today as symbols are depicted in ancient Egypt, they have nothing but their obvious and operative significance. There is no evidence whatever to the contrary.

Since our personal opinion, unsupported, of the Pyramid mystics may not be worth much, let us see what some others have said.

Petrie, *Seventy Years in Archaeology* :—

' The fantastic theories, however, are still poured out, and the theorists still assert that the facts correspond to their requirements. It is useless to state the real truth of the matter, as it has no effect on those who are subject to this type of hallucination. They can but be left with the flat-earth believers and other such people to whom a theory is dearer than a fact '.

In *Ancient Egypt*, part II, 1930, he also writes :—

' It need hardly be said to our readers that the extraordinary fallacies and misstatements about the Great Pyramid are lamentable nonsense. The prophetic theories which the writers elaborate are the substitutes for others of the past 60 years, always foretelling a few years ahead, and when disproved by events then shifted to new dates. A prophecy concerning 50 years hence would be safer but not so sensational '.



## PYRAMIDS AND THEIR PURPOSE

Borchardt's opinion will be found in that excellent publication of his, 'Gegen die Zahlenmystik an der grossen Pyramide bei Gise' (Berlin, 1922), which contains the clearest refutation of these mathematical delusions.

Budge, *The Mummy* (1925) :—

'According to some distinguished thinkers the arrangement of the chambers, the lengths and angles of the inclination of the corridors, etc. represented mysteries the knowledge of which was of the highest importance to the human race, and every measurement had its esoteric meaning and symbolism. The present writer is convinced that the Great Pyramid was built not to serve as an astronomical instrument or as a standard of measurements for the world, but as a tomb and as nothing but a tomb'.

He also wrote in *The Nile* (1902) :—

'It is well to state at once that the Pyramids were tombs and nothing else. There is no evidence whatever to show that they were built for purposes of astronomical observations, and the theory that the Great Pyramid was built to serve as a standard of measurement is ingenious but worthless'.

Arthur Weigall, *Ancient Egypt* (1928) :—

'It may be mentioned in passing that the modern theories attributing prophetic significance to the measurements and arrangements of the inner passages of the Great Pyramid are quite fantastic, and do not receive the support of Egyptologists'.

Reisner, *Mycerinus* (1931) :—

'Of quite a different character was the interest excited by the supposed mysteries of the pyramids in the group of writers led by Piazzi Smyth, whose disquisitions have never had any archaeological value and need no further mention'.

Finally, Arthur Mee, who is not an archaeologist, in his *Wonderful Year* has given us about the best statement of the position :—

'"The People Who Will Believe Anything" :—There are people rich beyond their dreams, but how many people there are who are rich beyond their brains ! It is pitiful to think that for every knave there is a fool, that every rogue can find a hundred simpletons to play on' . . .

There is really no particular reason why these mystics should have chosen or limited themselves to the Pyramid of Khufu. Borchardt, in the publication previously referred to, makes them a present of the fact that in the pyramid of Sahure's Queen at Abusir the proportion of half the base perimeter to the height is equal to Napier's logarithmic

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base 'e' (2.71828). We might go further and suggest that if the Crystal Palace were substituted for Khufu's Pyramid an enormous increase in the possible number of measurements would be found, and undoubtedly a great many of them would yield the exact values of a number of things. If a suitable unit of measurement is found—say versts, hanks or cables—an exact equivalent of the distance to Timbuctu is certain to be found in the roof girder work, or in the number of street lamps in Bond Street, the Specific Gravity of mud, or the mean weight of an adult goldfish.

To those who are familiar with Wilde's 'Portrait of Mr W. H.' that tale will present a good parallel to the mentality which has invented and maintained these astounding theories. It has been customary in some circles to dub them 'Pyramid-ites', but after all 'Smyrniot' and 'Cypriot' are used for 'those of Smyrna' and 'those of Cyprus', so why not Pyramidiot?



# The World-wide Expansion of Neolithic Culture

by A. VAYSON DE PRADENNE\*

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A PROBLEM is raised, in the first instance, by the remarkable similarity between certain stone implements, of a somewhat complex type belonging to the neolithic period of Europe, and those found in Asia and still in use in Oceania and America at the time of their discovery. For instance:—the triangular polished axes identical in France, New Guinea and Venezuela; the flint or obsidian knives, with parallel edges and triangular or trapezoid section, found in France, in the Greek islands and in Mexico; the mallets and axes with groove (*maillets et haches à rainure*) whose distribution extends from France and Spain to the Andes and North America, passing by Sinai; barbed and tanged arrow-heads, etc.

The first explanation that occurs is that this is a phenomenon of convergence: in each different region Man has been faced by the same problems and has solved them in the same manner. This simple theory might seem quite tenable. Nevertheless, in view of the number of possible solutions, such identity remains remarkable. Explanations which, taken singly, seem quite admissible, become altogether unacceptable when invoked too often. Thus, as a great mathematician once said, when playing cards to play the king is quite usual; but he who should play the king ten times running must be a sharper!

Let us then investigate the subject of inter-regional connexions during the neolithic period. Both the period itself and its limits in time are, to tell the truth, imperfectly known. But we do know that

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\* Translated by the Editor, who wishes to acknowledge his thanks to the author, the Institut Français d'Anthropologie (at whose meeting on 21 February 1934 this paper was read) and to the Editors of *L'Anthropologie*, where it was first printed, 1934, XLIV, 719-23.

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it was closely linked with the Bronze Age—so much so that one might wonder whether a pure neolithic period existed at all ; whether there were not just peoples with a common culture and way of living, certain of whom, from lack of raw materials, were almost or quite without metal implements. However this may be, the early part of the Bronze Age appears simply as a continuation of the neolithic, and for the period for which documentary evidence is available certain facts attract our attention. First, the uniformity throughout the Mediterranean-European zone, in spite of local varieties in the flint industry. Then, that retardation observable in the same zone whose western and northern districts always lag several centuries behind the eastern and southern ones.

At a slightly advanced stage in the Bronze Age intercourse between the Asiatic coasts of the Mediterranean and the Far East, through Persia and India, is first plainly observable. Similarly one can trace connexions between China, Siberia and central Europe on the one hand and northern Europe on the other. Granted that we are still without evidence of the true neolithic in these regions, yet the continuity between the two periods may perhaps justify us in interpolating it.

On the other side, the relatively recent settlement of Oceania, from the Indies and Indochina eastwards, becomes more and more certain. (The researches of M. Rivet concerning this point are well known). The resemblances between the neolithic tenon-axes of Japan, so highly specialized in type, and those of the Society Islands, the Marquesas and Alaska (Kadiak Island), in use down to modern times, are evidence of established relations. Axes of the same type, which went out of use in Japan more than two millennia ago, are still to be met with amongst the islanders of the east.

Finally, the connexions between the Eurasiatic and American continents are demonstrated by the existence of a great northern province. The nearness of the different land-regions throughout this zone, the sameness of the environment, the great migratory movements that go with a hunting régime—all combine to produce uniformity in the human population and its material culture.

But the hyperboreans of Asia, like those of America, were in touch with their continental neighbours. Though stranded on the margin of an evolving world, they availed themselves of opportunities to learn certain technical processes. Thus, for instance, an exchange of cultural traits has been observed between the Eskimos and the British Columbian Indians.



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That is not all. There are certain curious parallels between China and North America, in the use of stone implements, for instance the stone hoe, a tool whose method of use leaves clear and unmistakable traces behind it. M. Janse has recently recorded, in connexion with a neolithic excavation in Sweden, a type of pottery marked by identity of technique and ornament, and distributed from Scandinavia to North America, passing thither by way of northern Asia. Siren, too, has proved connexions between the ornament on Chinese pots of the Chao epoch and American pots. Andersson has pointed out resemblances in shape and ornament common to both Chinese and Central American pottery (tripod vases with feet bulging out at the top, double spiral ornament, anthropomorphic urns). On the other side, Andersson, according to Jackson, has shown the importance of the discovery of Oceanian cowries in pre-Columbian American graves. Heine-Geldern has called attention to the resemblance between Chinese neolithic oval knives with two holes and examples from Oceania.

New facts, all pointing the same way, are continually turning up. In the interesting New Caledonian exhibition just opened at the Trocadero, there are to be seen polished stone objects of very peculiar shape, of ancient origin and resembling certain specimens from North America and the Antilles (*pics à gorge avec bourrelets*, axes in one piece with the handle). Are these all examples of convergence? It would be strange if they were.

Doubtless it will be necessary to cast overboard some of the instances cited, but they are so numerous that some must surely remain over. Moreover, the list is not the result of an exhaustive search.

Facts such as these, revealing connexions between peoples distributed over huge areas and in a stage of development corresponding to our neolithic or chalcolithic, suggest that, before drawing any conclusions, we should consider in general terms how humanity achieves its progress and how civilizations develop uniformity.

During the Old Palaeolithic period there is clear evidence of the existence of a huge culture-area, or rather of a region, comprising about half the world. From the Thames to the Cape, from the Atlantic to the Indies, we find the same Chelleo-Acheulean industry, occurring under similar conditions. We have, then, to do with a set of conditions which lasted a very long time, but which taken as a whole may be regarded as having existed contemporaneously in each of the regions concerned. Thus we observe that this very ancient human



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stock had a uniform type of industry ; indeed apart from that we know little about it.

So too there was quite a remarkable uniformity in the industry of the Middle Palaeolithic. And, in spite of more complications, it does seem that in the Old World there was uniformity in the late (or Upper) Palaeolithic. At the same time, in this stage one notes the disappearance of that lower race which had so wide a dispersion during the preceding epoch.

In general one observes in several cases an industry reduced to uniformity and accompanied or not by the disappearance of lower races.

But today, on the other hand, one finds peoples of very different stages of development, from the neolithic to the machine age. They are strung out along a road five or ten thousand years long. But first one must note that the early undeveloped phases of civilization are less favourable to the establishment of relations between peoples than are some nomad or savage states. Man is tied to the soil he cultivates ; he marks out his field and barriers arise on every side. Sometimes these are overcome by commerce and war, but their influence and powers of isolation remain considerable. It is from such a state that we have just emerged. However, the few thousand years which divide us from the neolithic are of but a moment's duration compared with the great preceding epochs of cultural uniformity. Now at length a new process of unification has begun. The birth of the modern world of industrialism has set in motion a world-movement whose results, already partly achieved, will be the universal adoption, in a short time, of the new processes, and the disappearance of the races who cannot adapt themselves thereto. Machinism, while giving a new power to its creators, forces upon them at the same time a need of expansion. They invade the whole world, spreading by force or persuasion that material organization of theirs which they call ' the benefits of civilization '. The laggards have no time to develop ; they are carried off breathless into the new world, or are eliminated. A few decades or a few centuries hence the world of humanity will have been levelled out again, except perhaps for a few residual regions.

Let us now look again at what we know of neolithic times. We must first of all throw overboard that far too simple conception of the earlier prehistorians—that the main achievements of the neolithic period consisted in the polishing of stone and the making of pottery. To sharpen the edge of a pebble by rubbing it on sandstone instead of



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by knocking off flakes is not to achieve a revolutionary advance ; the best stone workers, in Scandinavia, employed at one and the same time axes of similar shape, the one kind polished, the other chipped. As for pottery, many peoples who know of it prefer less fragile receptacles made of wood or leather.

No ; the neolithic period is characterized by the domestication and breeding of animals on the one hand, and by the selection and cultivation of plants on the other. Thus a stable and assured mode of life took the place of one that was at the mercy of the luck of the chase and of the weather. Thus was animal-power captured and turned to the profit of humanity. That constituted a revolution comparable with the modern capture of natural energy by the use of machines.

On *a priori* grounds alone one might have expected that this revolution should have started a movement analogous to the one in which we are now ourselves involved, something like a tidal wave of world-wide extent submerging everything, annihilating the old world except for a few parts (*lambeaux*) here and there. The dense population of these regions, compared with that of the preceding periods, points in the same direction. The mesolithic has left so few traces that it remained undiscovered for a long time. The caves and sites of the Reindeer Age are as nothing beside the neolithic remains with which Europe is covered. This increase in population would seem to be the index of a considerable power of expansion. Whether this power acts by setting the peoples themselves in motion or whether it sometimes merely spreads the new mode of living, the result in either case is a levelling out of industry.

If such was really the case, all the facts we collect, the close analogies between the implements or arts of distant countries, instead of appearing unusual, will be seen to fall into place and be perfectly explained. It is all related, all is derived from one and the same movement, to an amplitude comparable with that in which we are involved, but one which submerged the Old World and doubtless discovered the New. The greatest difference lies in the rate of propagation which is now of the order of centuries rather than of millennia. The phenomena of world-wide lags—for historically the world appears as having only two limits separated by the Atlantic—are also of the order of millennia ; whilst those we meet with in the proto-history of our little Europe are of the order of centuries. For instance, between the New Guinea people with their polished stone axes and pile-dwellings and our pile-dwellers, there are a few thousand years. Between the

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Scandinavians of the end of the Bronze Age and the Mycenaeans there were only a few centuries. That is what might be expected.

To sum up :—the idea which emerges from the facts set out above is that, instead of regarding the polished-stone cultures of contemporary savages and our own neolithic ancestors as comparable—as the convergent outcome of separate lines of evolution, proceeding at unequal rates—one should regard them both as the outcome of a great shock that shook humanity and roughly levelled up the various industrial stages, being succeeded by a phase of isolated advances moving at unequal rates.



# Ancient Babylonian Maps and Plans

by ECKHARD UNGER

1. Assyriology is fortunate enough to possess, in Babylonian and especially in Assyrian royal inscriptions, architectural descriptions often quite detailed in nature. A case in point is the full account of the reconstruction of the Temple of Assur in the city of that name undertaken by Sennacherib toward 700 B.C., when he rebuilt several of its gates. These 'new gateways of the Temple of Assur' figure again in a well-preserved 'Description of the city of Assur'. Together, then, with the account of Sennacherib's rebuilding, a contemporaneous description of Assur, giving in catalogue form, in 183 lines, the main features of the ancient city, has also come down to us. And I have succeeded in piecing together a number of fragments which together supply 268 lines of a 'Description of Babylon' of a similar nature.<sup>1</sup> Apart from descriptions of buildings and cities we also possess cartographic remains depicting the earth, landscape, fields and buildings projected as seen from above. These ground-plans,<sup>2</sup> which form a very welcome corroboration of the written texts, are chiefly found on clay tablets—the usual stationery of Mesopotamia. Generally amplified by text, they deserve special attention, for they supply valuable material for the study, not only of ancient topography and buildings but also of early ideas of how man orientated himself in his world.<sup>3</sup>

The earliest ground-plans appear on cuneiform tablets (FIGS. 1-2) bearing deeds of sale of property, of which the definition of shape and

<sup>1</sup> Unger, 'Das Stadtbild von Assur': *Altaer Orient*, 27 (1929); *id.*, 'Assur (city)': *Reallex. der Assyriologie* 1, p. 182, § 37, b, 4; Schroeder, *Keilschrifttexte aus Assur hist. Inhalts*, II, no. 124; Unger, *Babylon nach der Beschreibung der Babylonier*, 1931, Document no. 1; *id.*, 'Babylon': *Reallex. d. Assy.*, 1, pp. 330 ff.; *id.*, 'Topographie der Stadt Dilbat': *Archiv Orientalni* III (1931), p. 46 f.

<sup>2</sup> Summary: Unger, 'Tabula', in Ebert, *Reallex. der Vorgeschichte*; also, E. D. van Buren, *Clay Figurines of Babylonia and Assyria*. Yale Oriental Series, XVI, p. LXI, 273-6 (16 maps, nos. 1309-24); and Meissner, 'Babylonische . . . Landkarten': *Klio* 19 (1924), p. 97 f.

<sup>3</sup> I hope soon to publish, under the title *Welt und Mensch im Alten Orient*, a work on archaic conceptions and on the development of ideas in early eastern culture.

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location was a practical necessity. They occur on some Fara tablets, but the drawing on these is of course very primitive. Paucity of material prevents us from tracing in detail the history of Babylonian cartography,

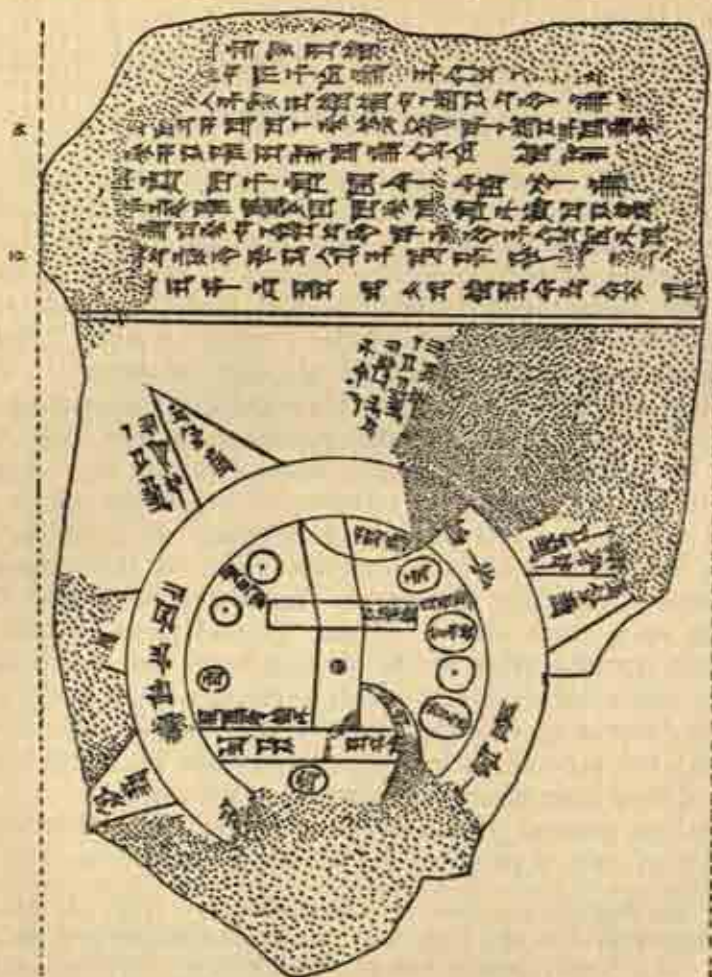


FIG. 2. MAP OF THE WORLD, AFTER A DESIGN ON A CUNEIFORM  
TABLET (See FIG. 1)

*By permission of the British Museum*

but so far as we can yet tell methods have not greatly changed. Objects—in this case the ground-plans—were projected as seen from above by means of straight strokes. Just as in the very early cuneiform curved lines are very rare, so in these ground-plans too they are usually avoided,





FIG. 2. MAP OF THE WORLD ON A CUNEIFORM TABLET, BRITISH MUSEUM

(*'Cuneiform Texts'*, xxi, plate 48)

*By permission of the British Museum*





## ANCIENT BABYLONIAN MAPS AND PLANS

for the clay permitted the scratching of angular forms but was too stiff for curved ones. This explains why rivers and canals are seldom represented by winding curves but nearly always by angular forms. Sometimes, however, canals, indicated by two parallel lines, were filled in by wavy ones (FIG. 6), an expressive way of rendering water [a convention still used by cartographers and called 'water-lining']; but usually the space was left blank and the name of the canal inserted, the

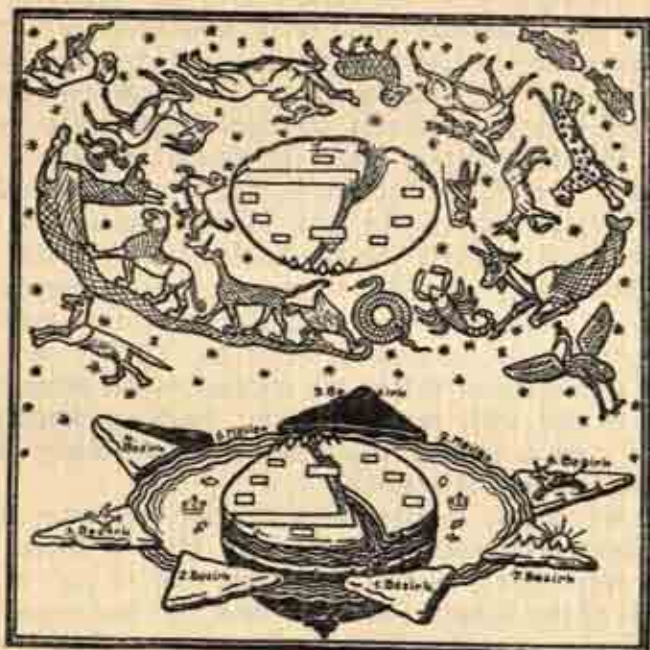


FIG. 3. RECONSTRUCTION (BY UNGER) OF BABYLONIAN MAP OF THE WORLD  
From 'Atlantis' (Berlin) April 1932

addition of the designation 'canal' removing all ambiguity. Ground-plans of houses were executed on a system still used today: the various thicknesses of wall were indicated and the doors marked by openings. If the draughtsman made a mistake he would, say at a place originally marked as a door, put a diagonal cross— $\times$ —to show that the opening was 'invalid', 'non-existent', 'crossed out', just as was done for cancelled clay tablets or 'unsafe' squares on gaming-boards.<sup>4</sup> This

<sup>4</sup> Unger, 'Spiel': *Reallex. der Vorgeschichte*; Clay tablet in Berlin (VAT 8738); *id. Babylonisches Schrifttum*, 1921, fig. 10; Ground-plan in Berlin: Borchardt, *Sitz. K. Preuss. Akad.* 1888, pl. 1; Meissner, *Babylonien und Assyrien* 1, fig. 154.

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may even have indicated an alteration made in the course of construction, the plan having served for the original lay-out.

2. The only example of a Babylonian world-map hitherto found dates from the Persian period and is probably part of a comprehensive description of the world. According to the text on the tablet we have here a chart of the 'Seven Islands' or regions supposed to lie between the 'Earthly Ocean'—called the 'Bitter River'—and the 'Heavenly Ocean'. A description of the latter, with its zodiacal signs, even comes into the text. Yet the round earth, with Babylon as its centre, the 'hub of the universe', is just roughly sketched in, being only required, apparently, to make clear the position of the 'Seven Islands'. The ideas connected with these islands are reproduced in the reconstruction in FIG. 3. The fifth region bears the significant note: 'Where the sun is not seen'; it lies in the north, therefore, and the Babylonians knew of the Polar night. In the fourth region 'semi-obscurity reigns', in the sixth dwells a 'hostile horned bull', in the seventh the sun rises.<sup>5</sup> For a long time this cosmos was the accepted idea of the world, but every nation saw it with a different centre—itsself, placed, as the case might be, in Jerusalem, Egyptian Thebes or Greece, and naturally the arrangement within the cosmos varied accordingly. That an astrolabe of the 17th century of our own era displays a similar 'horned bull', on a similar triangular field, similarly placed, bears witness to the tenacity of tradition.<sup>6</sup>

3. District maps, mostly from Nippur, showing fields, canals and places, have also come down to us. One of these, of which a considerable portion is preserved, is a local map of part of the Nippur area, probably of the Kassite period (1300 B.C.),<sup>7</sup> and shows a canal and the town of Hamri, as well as Kār-Nusku and several other places. We possess a similar but very fragmentary map of the Lagash [Tello] district dating right back to Akkadian times (2700 B.C.). Mention must also be made of the city of Sippar shown upon a district map in which the Euphrates and the Tabbishtum canal appear by name. Possibly this is the Sippar of Shamash, the Sun-god, the ruins of which were uncovered at Abū Habba.<sup>8</sup>

<sup>5</sup> Unger: *Babylon*, figs. 3 and 4, document no. 6, p. 254 ff.; *id.*, 'Rekonstruktion des Kosmos': *Atlantis*, 1929, p. 701; 1932, p. 246 f.

<sup>6</sup> E. G. Happel, *tomii*, *Andrer Teil grössester Denkwürdigkeiten der Welt*. Hamburg, Thomas von Wiering, 1685. The reconstruction has been based on this.

<sup>7</sup> In Philadelphia; Langdon, *Museum J.*, 7 (1916), p. 263 ff.

<sup>8</sup> Lagash: Thureau-Dangin, *Recueil de Textes Cunéif.* no. 154. Sippar: *Cun. Texts*, XXII, 49 (B.M. no. 50644); Meissner, *op. cit.* II, p. 378.





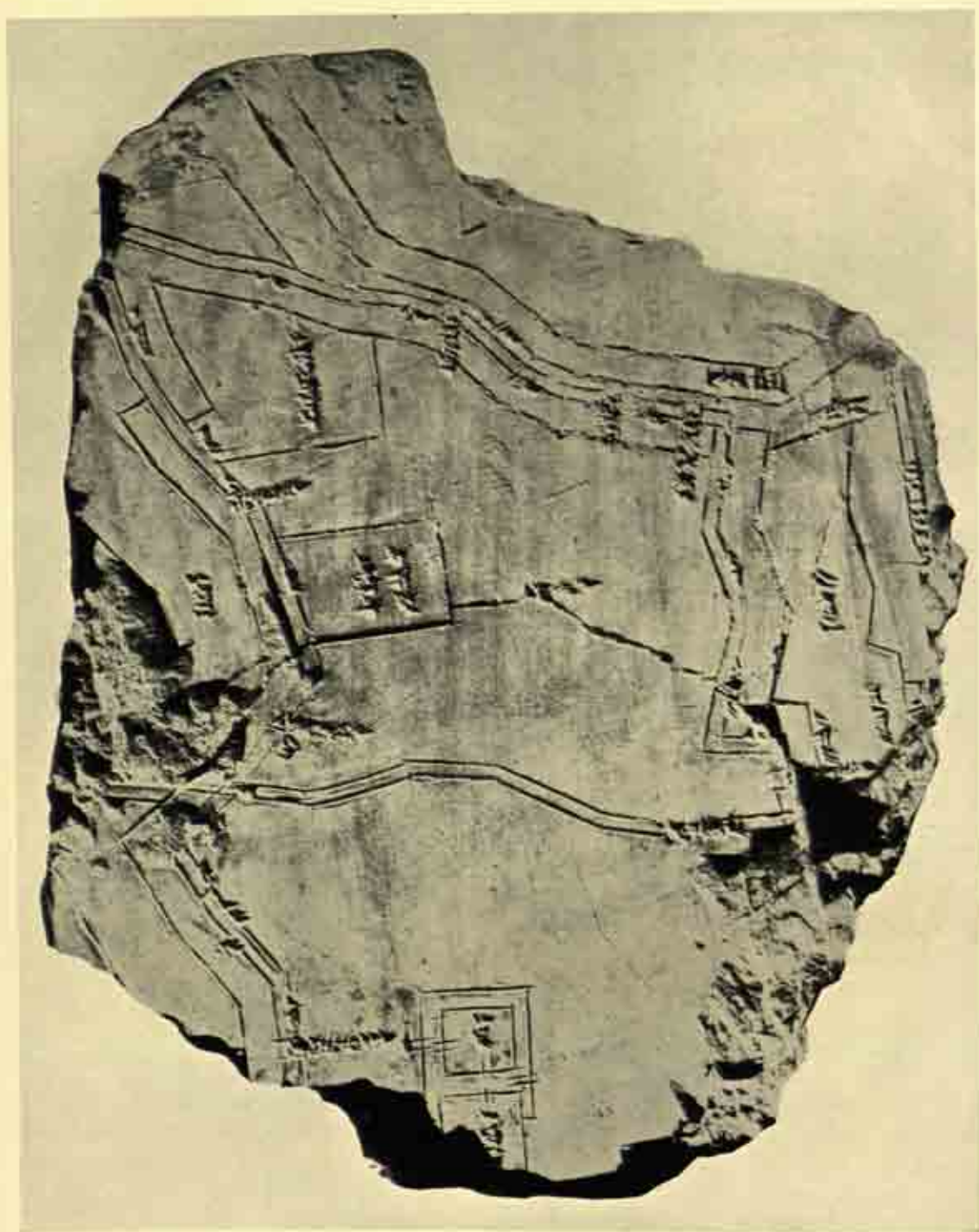


FIG. 4. CLAY TABLET WITH PLAN OF NIPPUR.  
From "Excavations at Nippur", by Clarence S. Fisher. Philadelphia, 1903



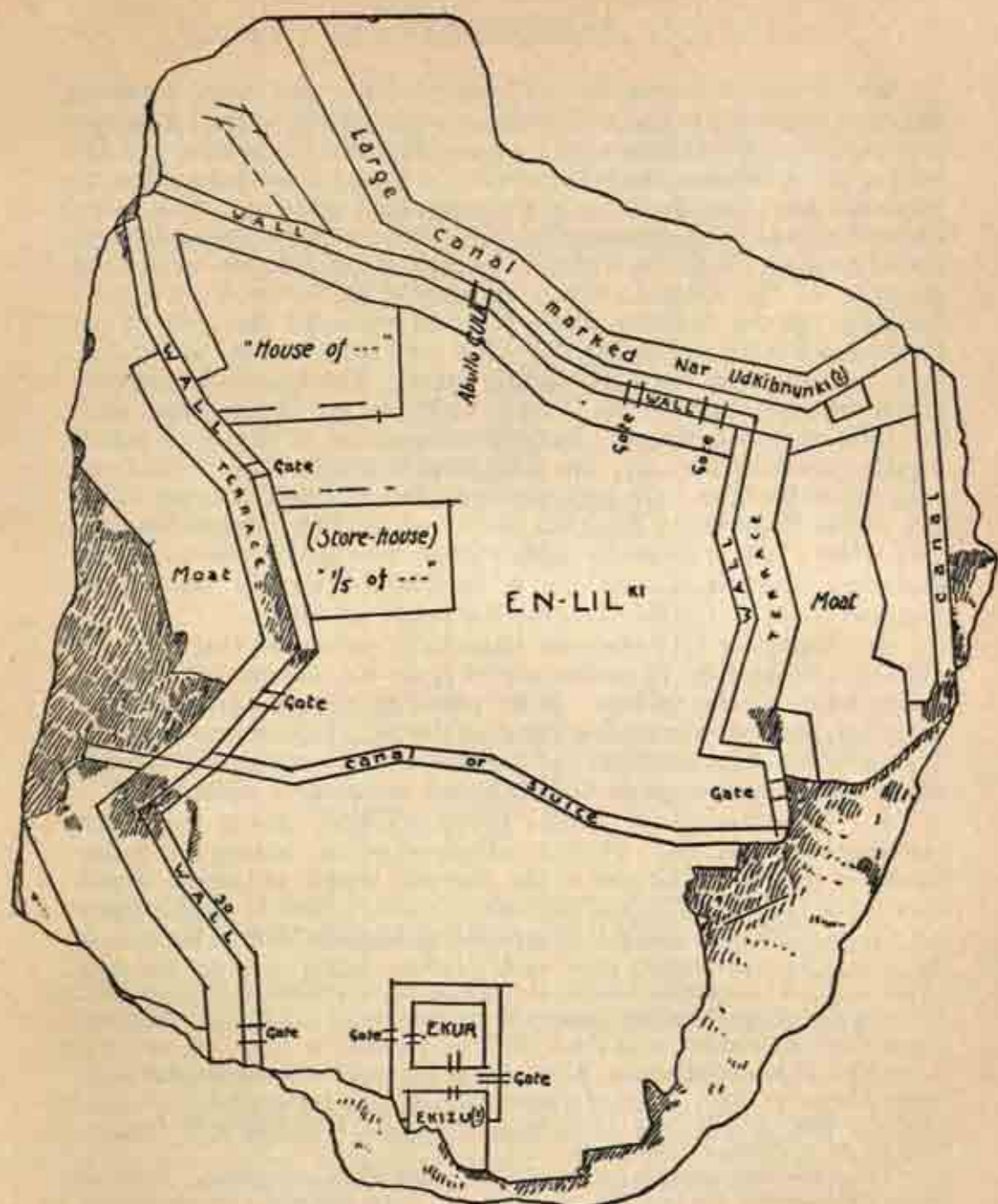


FIG. 5. DIAGRAM TO EXPLAIN PLAN OF NIPPUR, FIG. 4

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4. A number of fragments of town-plans have survived. Of these the most extensive is one of Nippur, which excavators claim to represent part of the city (FIGS. 4-5). I have identified two portions of the town-plan of Babylon, one showing the [Southern] Citadel, the other the Shamash gate and the suburb Tuba of the 'new city' of Babylon situated west of the Euphrates (FIG. 6). This plan of the 'new city', by the way, agrees absolutely with the 'Description of Babylon' mentioned above.<sup>9</sup> At the Citadel, which apparently represents a very early building, are the Arakhtu canal—a sacred name for that part of the Euphrates flowing within Babylon—and the 'King's moat' or Citadel-moat, traced along the Libil-hegalla canal. The inscribed measurements too seem right for the Citadel, which did not change much until the time of Nabopolassar. But Nebuchadnezzar II (600 B.C.) added outside, north of the city, two new citadels which we know did not exist before his time. On a town-plan of Assur appears an unidentified city on the Euphrates; King has found part of Sippar-Jahrurum on a clay tablet; and a fragment dating from Neo-Babylonian times and showing a 'Temple of Enlil' and a 'beginning of a street' may represent part of the city of Babylon, but this is not certain.<sup>10</sup>

5. Estate- or field-plans are remarkably numerous, even from the Akkadian to the Late Sumerian period (2700 B.C. downward), and are mostly found on clay tablets. In the post-Kassite period (12th century B.C.) such plans are sometimes found on the boundary-stones (*kudurru*). The individual measurements and indications of direction noted thereon afford a considerable guide to Babylonian methods of orientation.<sup>11</sup>

6. Ground-plans of houses probably figure among the oldest cartographical remains. They are chiefly of private houses but occasionally of temples. The plan of the Ningirsu temple at Lagash, drawn on a brick held in the lap of the seated diorite statue of Gudea (2600 B.C.), is particularly original. It shows only the outer walls of the temple complex, the two towers over each gateway being very prominently

<sup>9</sup> Nippur: King, *Sumer and Akkad*, p. 87; Van Buren, *op. cit.* fig. 316. Babylon: Unger, *Babylon*, Document no. 4 ('new city'), no. 5 (citadel), p. 253.

<sup>10</sup> Maps of Assur: Schroeder, *Keilschrifttexte aus Assur* no. 25 (Berlin, VAT 9423). Sippar-Jahrurum: King, *Letters of Hammurabi* pl. 242, no. 107, and vol. III, p. 255; Langdon, *Amer. J. Sem. Lang.* 39 (1922), p. 206; Enlil Temple tablet in London: B.M. no. 73319; *Cun. Texts*, xxii, 49.

<sup>11</sup> Thureau-Dangin, *op. cit.* nos. 149-60, etc.; *Cun. Texts*, I, x (*passim*). Boundary Stones: Steinmetzer, *Kudurru*, nos. 13, 15, 26, 79; King, *Babylonian Boundary-stones*, nos. 13, 15, 26.



## ANCIENT BABYLONIAN MAPS AND PLANS

projected and looking as if they were perpendicular to the temple walls.<sup>12</sup> On a couple of fragments of a tablet in London is the ground-plan of a building drawn in such detail that the brickwork is marked out and the measurements shown in ells and inches.<sup>13</sup> An interesting clay tablet from Babylon in Berlin bears a sketch-plan recording the results of the measurement of a house-plot near the Zababa gate;<sup>14</sup> probably this

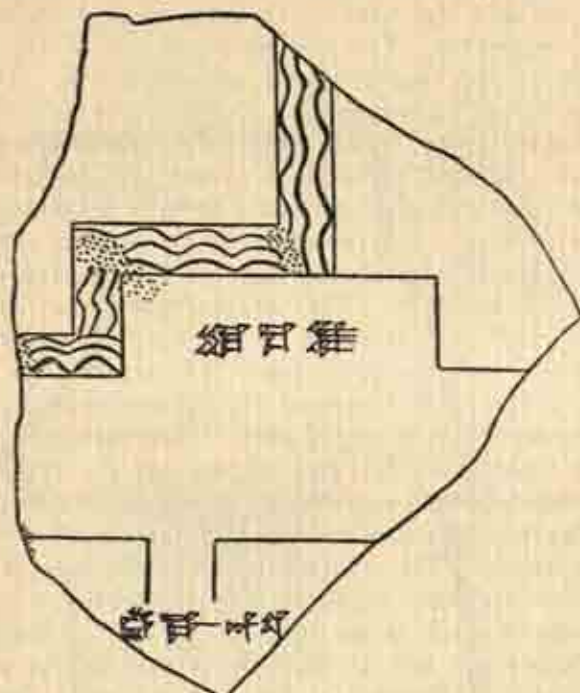


FIG. 6. FRAGMENT OF A MAP OF BABYLON ('NEW TOWN'), AFTER UNGER  
The plan is drawn with the southeast at the top. The water-lined band is the Euphrates  
in the middle is the place Tuba; all below it is called Samas-Tor

tablet was appended as an illustration to a deed of sale. I have already drawn attention (cf. § 4) to another big house in Babylon complete with measurements and with a diagonal  $\times$  marking a 'non-existent' door.

7. Mention has been made above (cf. § 4) of plans of city walls. Properly speaking they count as town-plans. It is not impossible, however, that they are concerned only with the representation of city

<sup>12</sup> Sarzec and Heuzey, *Découvertes en Chaldée*, pl. 15, 1 (statue B).

<sup>13</sup> *Cun. Texts*, XXII, 50, p. 13.

<sup>14</sup> VAT 68: *Vorderasiatische Schriftdenkmäler*, VI, no. 264.

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walls. A map of this nature, from Lagash, dating from late Sumerian times (2400 B.C.) has been preserved.<sup>13</sup>

8. The sketch-maps of camps on the Assyrian reliefs also come within cartography. They are first found, so far as we know, in the 9th century B.C., that is to say, on the reliefs of Ašurnasir-pal (880 B.C.). His successors emulated this style of plan. The camps are drawn as seen from above, and the towers are seen from within and projected from the camp outward. For similar treatment of the gate-towers in the temple plan of the Gudea statue, cf. above, § 6. The camps were always laid out on an identical plan, conforming to a military pattern and without individuality. The only difference found is that they are sometimes shown square, sometimes round, sometimes oval.<sup>14</sup>

9. These maps and plans all had a definite orientation. Examination of the world-map, for instance (§ 2), shows, especially by the 'dark' fifth region in the north, that in Babylonian orientation the northwest was at the top. The fragmentary town-plan of Nippur (cf. § 4) shows such orientation. In ordinary reproductions this clay tablet was formerly incorrectly placed. The orientation may be judged by the position of the name 'Nippur' in the centre of the map, which in King's reproduction is correctly placed. Now compare it with Fisher's reconstruction embodying the part shown on the fragment, and it is clear that the plan followed a system of orientation having the northwest at the top. The north arrow on the last-named reconstruction shows the exact orientation. The ground-plans of Babylon too must follow a similar—to our modern idea—inclined or oblique orientation, but either the key-word, such as we have in the map of Nippur, is lacking, or else these plans are not so well preserved as the world-map, for instance, the position of which on the clay tablet is absolutely certain. At any rate its inclined orientation is certain, though it cannot be definitely stated whether such orientation was due northwest.

If the maps be examined in detail it will be seen that the draughtsman, when he had drawn in the rectangular ground-plan of his building, would sometimes approach the plan from outside and add from each edge his notes of measurements, names and direction. Thus when noting the direction the draughtsman was looking, not towards the particular line of orientation but along, *i.e.* in the same direction as it. For the rest, he assumed four different view-points, approaching each

<sup>13</sup> Thureau-Dangin, *op. cit.* no. 147.

<sup>14</sup> Unger, 'Feldlager', in *Reallex. der Vorgeschichte* III, p. 202 f., pls. 42-43.



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side and writing along it. Clearly, it would be impossible definitely to ascertain the absolute orientation followed unless, as in the two cases under discussion, another important and decisive factor were present. The pre-requisites, then, are either a well-preserved tablet and the possibility of determining the orientation by the position of its text, or

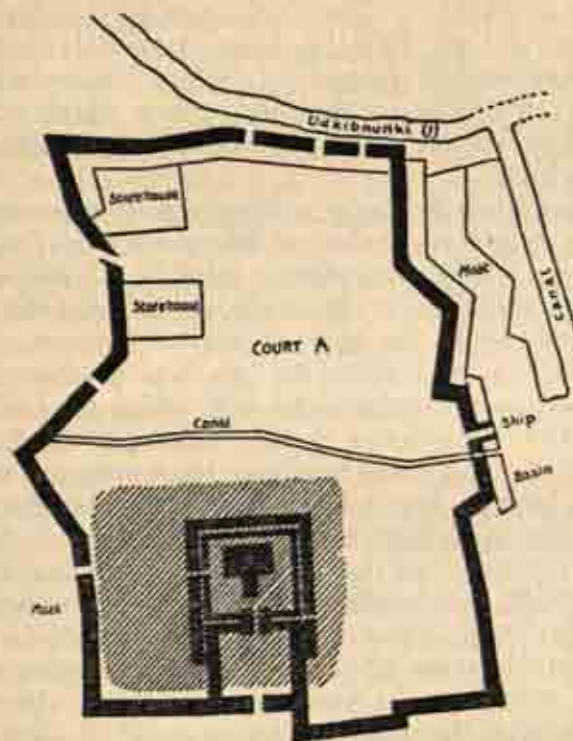


FIG. 7. PLAN OF NIPPUR, AFTER EXCAVATION. AFTER FISHER  
Compare with figs. 4 and 5

else the presence of the key-word to the map ('Nippur') together with one of the four independent side-orientations.

10. These two examples (cf. § 9) permit the unprejudiced conclusion that the Babylonian system of orientation was inclined, *i.e.* had the northwest at the top, and did not follow the perpendicular plan—north, west, south and east—of our cardinal points. Four years ago I was able to produce evidence to show that the Sumerians originally based their system of orientation on the direction of the prevalent

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winds in Mesopotamia. Meteorological observations conducted over the past 150 years have established these winds as approximately NW, NE, SE and SW. The Sumerians determined the individual character of these four prevalent winds and interpreted them as manifestations of gods of like nature. The manifestation of deities through the direction of the wind played an important part in the orientation of the respective temples set up to them.<sup>17</sup> The fact must be emphasized that the winds in question do not blow from one fixed point but rather from some point approximately comprised within the arc of the relevant quadrant, as my windrose in the town-plans shows.<sup>18</sup> Orientation followed, therefore, not one definite line but a direction contained within the arc of a quadrant.

The northwest wind, the so-called 'favourable wind', was the manifestation of beneficent gods—of Ishtar [at Uruk], the goddess of love, for instance; while the southwest wind, the 'storm-wind', which brought sickness and disaster from the desert, was the wind of the demons of illness and of the gods of war. The southeast wind, the 'cloud-wind', which blew from the sea, was dedicated to the gods dwelling in the sea, in the underworld, with which they had connexions. The northeast wind was known as the 'mountain wind', and had no special associations. But in the northeast the sun rose at the time of the summer solstice (our 21 June). This, therefore, was the time selected for determining the orientation of the city of Babylon. The northwest walls, and the main street of the city's god, of Marduk, the god of the 'sun of day', which ran parallel to these walls, were oriented toward this point in the sun's course. Thus was obtained an astronomical time-gauge, occurring once a year, when the sun shone straight down the street, and providing the basis for calculating the normal lunar calendar and adjusting the solar year by means of an intercalary month. In the neighbouring city of Borsippa the procession street was likewise oriented astronomically by the appropriate phases of the moon.<sup>19</sup> So a system of orientation by the stars as well as by the winds was employed. This system was an inclined one too and never followed that of our cardinal points, which shows that although astronomical

<sup>17</sup> Unger, in *Forschungen und Fortschritte*, IV (1928) p. 343 f., V, (1929), p. 270 f. *Babylon*, p. 122-135; see also footnote 3 above, where this matter is further discussed.

<sup>18</sup> Unger, *Babylon*, fig. 27, pl. 18.

<sup>19</sup> Unger, in *Atlantis*, 1932, p. 244 f.; *id.*, *Babylon*, p. 382; Borsippa: *id.*, 'Borsippa', *Reallex. der Assy.* I, p. 418, § 57.



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considerations were taken into account, the tradition whereby a particular deity was manifested through the wind held first place. In all available material hitherto studied the orientation of the temples and the character of the gods to whom they were dedicated—at any rate in Babylonia—correspond with the direction and nature of particular winds. Theoretically, orientation was and remained fixed within the arc of a quadrant. Whether in practice, however, a temple could be oriented on the day of its foundation according to the 'officially fixed' direction of the wind—which might at the time be variable—may be doubted. The theory, then, that the Babylonians actually based their system of orientation on certain stars, is, as I shall attempt to prove on other grounds, quite a plausible one. These stars shone fairly regularly on certain days within the wind-quadrants definitely fixed by tradition. If this assumption is correct, then the wind-quadrants were fixed on certain general lines, and in practice orientation would be based, not on the actual direction of the wind—since it might be variable—but on observation of the stars, assuming, of course, that they appeared within the limits of a given quadrant. The orientation, in theory, of our churches to the East, not to Jerusalem, and, in practice, of mosques to Mecca, illustrates this point.

The terms 'sunrise' and 'sunset' in Mesopotamia imply a similar orientation by quadrant. These terms did not denote the direct easterly or westerly cardinal points as we understand them, but the complete quadrants within which the sun rose and set. Thus Mount Bikni, where lapis-lazuli came from, and which has been identified with Mount Demavend in the northeast of Assyria, was called the 'mountain of the rising sun'. But the Persian Gulf, situated in the southeast of Babylonia, was similarly named the 'sea of the rising sun'. So when King Sennacherib, in 700 B.C., spoke of the two walls of Nineveh—the one orientated to the 'mountain-wind' (NE), the other to the 'cloud-wind' (SE),—as 'turned to the rising sun', he was being quite consistent. This also goes definitely to prove that while the 'mountain-wind' and the 'cloud-wind' blew from the NE and SE quadrants respectively, they must not be equated with exact east or south. But when it was a question of defining an astronomical direction, the Babylonians, owing to the need for greater precision, may quite well have departed slightly from traditional practice. All that this would mean, however, is, that while a quadrant is still indicated, the 'mountain-wind' (NE) includes the due east wind, the 'cloud-wind' (SE) the south wind, the 'storm-wind' (SW) the west wind, and the 'favourable

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wind' (NW) the north wind.<sup>20</sup> We must await proof from astronomical texts as to when this method commenced. From cuneiform texts of the Greek Diadochi period (3rd century B.C.) we learn that the term 'sunrise' was employed for the 'mountain-wind' (NE), and 'sunset' for the 'storm-wind' (SW). This already reveals a certain narrowing of the earlier conceptions of 'sunrise' and 'sunset'.

Excavations have repeatedly proved the inclined orientation of buildings. Further, inscriptions on dedication tablets show that the inclined orientation was intended to correspond with the direction of the wind. In studying Babylonian maps and plans it is of vital importance to know that they are thus obliquely oriented just as the noteworthy buildings were. Only, however, by studying together both the actual buildings excavated, and the texts, can ancient Babylonian plans be fully understood.

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<sup>20</sup> The above quadrants may quite well be used to indicate astronomical direction. The quadrants to N, E, S, and W, postulated by Neugebauer and Weidner (*Archiv. f. Orientforsch.* VII, p. 269 f.) are diametrically opposed to the evidence of the buildings by which I have gone but which both these writers simply ignore. Their objection to my alleged equation of the NE direction with 45°, and the SE with 135°, and so on, does not trouble me, as I never assumed these to be exact points but only points within an arc, as my Babylon, Borsippa, etc., windroses show (cf. note 18).



## The Poems of Llywarch the Aged\*

by KENNETH JACKSON

MANY readers of ANTIQUITY know Skene's translations of the early Welsh poems from what he called the 'four ancient books of Wales',<sup>1</sup> but not so many, perhaps, realize that though his work was a considerable piece of scholarship in his day his renderings are now quite out of date and nowhere to be trusted by the layman. Among these poems are a number of great interest and importance for the study of the Dark Ages in Britain, treating the westward expansion of the English from the Welsh point of view; and Celtic scholars could be of great assistance to the archaeologist and historian by editing and translating them afresh in the light of modern knowledge. A beginning, a preliminary clearing of the ground, was made by Sir John Morris Jones in his study of Taliessin<sup>2</sup>. Now Professor Ifor Williams, with his profound knowledge of early Welsh language and literature, has settled the problems of the cycle attributed to Llywarch Hên in a way not likely to be revised for a long time. As the book is written in Welsh a rather full summary is called for.

*Canu Llywarch Hên* ('the poems of Llywarch the Aged') comprises Williams' edition from all known manuscripts of every early poem attributed to or connected with Llywarch, with a very complete commentary on the linguistic and textual difficulties; and an introduction dealing more fully with the historical aspects already briefly discussed in his 'Poems of Llywarch Hên'<sup>3</sup>. Up to the present these poems have been very largely unintelligible, partly for their linguistic obscurity but still more because their scheme of arrangement was not understood. Professor Williams has brought order into this chaos by a theory which is perhaps the most brilliant aspect of the whole book. As the poems

\* *Canu Llywarch Hên*. By Ifor Williams. pp. xcii, 266. Cardiff: University of Wales Press, 1935. 10s 6d.

<sup>1</sup> W. F. Skene, *The Four Ancient Books of Wales*. Edinburgh, 1868.

<sup>2</sup> *Y Cymmrodor*, vol. 28.

<sup>3</sup> *Proc. Brit. Acad.*, vol. 18, Rhys Lecture for 1932.

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appear in the Red Book of Hergest, the earliest extant manuscript, they cover about  $12\frac{1}{2}$  columns (1034-1042, 1044-1049), divided by large capital headings into five separate poems.<sup>4</sup> Williams shows—and this is indeed obvious, though no one appears to have noticed it before—that there are really considerably more than five, but that with these exceptions they have all been run together without any indication where one poem ends and the next begins. Thus the first 21 verses of the poem beginning on col. 1036<sup>5</sup> are a monologue by Llywarch lamenting his old age; the next 13 are a dialogue between his son Gwen, himself, and his wife; and the next 14 are an elegy by Llywarch on the dead Gwen; yet there is nothing in the set-out of the manuscript to show that this is so—to tell for example that Gwen was alive in the second section and dead in the third, or how he had come by his death. Williams accounts for this, and in doing so solves the whole tangled problem, by his theory that these are the verse-portions of a story which was told originally in verse and prose; and that since the story-teller had to remember the verses accurately for metric reasons, they alone were written down, while the prose parts were left unwritten because it was easy to remember their drift and unnecessary to repeat them each time in precisely the same form. This kind of mingled verse and prose tale is very rare in Welsh literature, but Williams is able to quote a satisfactory and conclusive parallel. The type is of course a regular one in Irish. On this basis he reconstructs the whole tale, relying as much as possible on the internal information of the poems and as little as possible on conjecture.

Professor Williams shows that the poems belong to two independent cycles: the story of Llywarch Hên and the quite unconnected traditions about Cynddylan and the other descendants of Cyndrwyn. Llywarch appears in the early genealogies as first cousin of Urien, the prince of the northern Britons, who is known from Nennius to have lived in the late 6th century and to have opposed Theodric and the invading English settlers in Northumbria.<sup>6</sup> Williams re-affirms the general trustworthiness of these genealogies and disposes (let us hope for good) of the unfortunate theories that they, along with much else of our sources for early Welsh history, are forgeries of a later date. The poems in question illustrate this genealogical evidence and do not conflict with it in any way; and they contain practically all the other early information about Llywarch which we have. He appears first as

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<sup>4</sup> Skene, I, 326, 355, 448, 580, 584.    <sup>5</sup> Skene, I, 326.    <sup>6</sup> ed. Mommsen, p. 206.



## THE POEMS OF LLYWARCH THE AGED

the speaker, not named but clearly intended, of an elegy or elegies on his cousin Urien of Rheged. We hear of a faction between Dunawd<sup>7</sup>, Gwallawg (Guallauc in Nennius), a certain Bran, and Morgant (Morcant the betrayer of Urien in Nennius), against Urien's sons; and learn that Urien has been killed at Aber Lleu,<sup>8</sup> presumably as a consequence; that his home lies in ruin and the greatness of Rheged is no more. In the other poems Llywarch has become characteristically the Old Man. One<sup>9</sup> appears to contain a dialogue where someone objects to going to battle because it is too cold, with fine descriptions of wintry weather, leading up to a dialogue between a son of Llywarch and a dependant of Owein son of Urien who is acting as his guide; and ending, presumably after a lost prose-link, with an elegy by Llywarch on this son killed by a certain Mwng Mawr-Drefydd. Others are advice to his son Maen,<sup>10</sup> a random collection of verses about his sons,<sup>11</sup> and similar verses from late MSS. not given by Skene (one with an explanatory passage of prose exactly of the kind postulated by Williams for the whole poetic cycle). One poem<sup>12</sup> introduces us to Gwen, the last of the 24 sons, who has returned from a long absence and now declares his intention of guarding the ford of Morlas (? near Oswestry) against the English; followed by Llywarch's elegy telling how Gwen 'watched last night by the ford of Morlas' and was killed. An elegy on another son, Pyll, some scattered stanzas on the graves of the sons, and four verses about three of them, Talan, Dwg, and Cyny, are added at the end. Another<sup>13</sup> is Llywarch's famous elegy on his own old age, expressed with extraordinary tragic power and restraint. The poem probably last in order (not in Skene) shows the aged Llywarch an exile in Wales; he meets a horseman who warns him against his old enemies Dunawd and Bran, reminds him that Urien's sons are all dead, and invites him to take refuge at Llanfor near Bala in Merioneth. It is not explained how it is that Llywarch and his sons, whose original connexions were with Rheged on the Scotch border, came to be settled in mid-Wales, and Williams does not attempt to account for it. The poem about the cuckoo of Aber Cuawg,<sup>14</sup> has nothing to do with Llywarch and belongs to some different story now unknown.

The orthography and scribal errors in the MSS. show that the earliest traceable exemplar must have been written in Hiberno-Saxon

<sup>7</sup> d. 596, *Ann. Camb.*

<sup>8</sup> cf. Ross *Low* opposite Lindisfarne, Nennius' *insula Metcaud.*

<sup>9</sup> Skene, I, 321.

<sup>10</sup> Skene, I, 584.

<sup>11</sup> Skene, I, 319.

<sup>12</sup> Skene, I, 326, vv. 21 ff.

<sup>13</sup> Skene, I, 326 vv. 1-20.

<sup>14</sup> Skene, I, 580.



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script and Old Welsh spelling some time before the Norman Conquest. Linguistic considerations suggest that it was much before, and the occurrence of three verses in precisely the same metre, language and style as the Llywarch poetry, in a 9th century MS., points to the existence of this body of verse as early as that date. For linguistic and other reasons it is hardly likely to be earlier. That is, the poems were probably composed soon after Nennius' *Historia Britonum*. It will appear that they drew to some extent on the same group of tradition, a tradition preserved independently by the genealogies and the *Annales Cambriae*.

The second cycle, that of Cynddylan and his family, is comprised in the long series of englynion in Skene (I, 448 ff), and takes the form of a dramatic monologue spoken by Cynddylan's sister Heledd. Cynddylan, son of Cyndrwyn and king of Powys, has been killed by the English and his town and palace have been laid waste. Heledd laments his death and that of her other brothers, describes the ruined hall and the eagles feeding upon the dead, envies her sister Ffreuer who has escaped her misery in death, and so comes to her own plight as a destitute wanderer herding goats in the mountains. The poem includes the well-known stanzas on the 'Hall of Cynddylan' and on Tren, 'the white town in the bosom of the wood', where the sense of tragedy heightened by restraint appears at its finest; and many lines such as, 'long is the sun's course, longer are my memories', (v. 80. II), illustrate well the early Welsh faculty for dramatic contrast and verbal economy. It used to be thought that this poem referred to the western campaign of Ceawlin in A.D. 577; Cynddylan was identified with the *Condidan* whom he killed;<sup>15</sup> and a passage in the poem supposed to refer to Uriconium was taken to show that this was the occasion of the English advance into that part of Britain. But Professor Williams has discovered a verse in a late MS. which states definitely that Cynddylan was an ally at the battle of Maserfield in A.D. 642, and an early poem on him called 'the Elegy of Cynddylan' which mentions his brothers as fighting before Lichfield. This points rather to the period of the struggle between Northumbria and Mercia when the Welsh under Cadwallon were the allies of Penda. In any case the actual poems are almost certainly later than Cynddylan's time, and appear to date, like the Llywarch cycle, in the middle of the 9th century. Analysis of the place-names, so far as they can be identified with certainty at all, suggests that they belong to two distinct areas. The first seems to be the

<sup>15</sup> *Comail*, p. XXXIII, is presumably a misprint; the MSS of the Anglo-Saxon Chronicle give *Coinmagil*, *Coinmail*, and *Coinmael*.



## THE POEMS OF LLYWARCH THE AGED

Shrewsbury district; Cynddylan's court is probably at Shrewsbury; the rivers Tern and Roden, which flow into the Severn just below Uriconium, are mentioned, and Cynddylan's 'white town', Tren, has the same name as the river. The Wrekin is apparently referred to, and Cynddylan seems to have been buried at Baschurch. The second area is well inside the border, in the present Montgomeryshire and Merioneth-Denbigh. The poet must have been very familiar with it. Williams thinks (pp. xc-xci) that the place-names belong to the geography of the 9th century, when Wales was confined to very much its present boundary and Mathrafal in the middle of our second district was the capital of southern Powys. But the first district was far outside Wales long before the 9th century, and in fact must have been absorbed into England soon after Cynddylan's time; and we might suggest that the poem preserves two strata of the tradition, the first belonging to Cynddylan's period when he was known to have ruled from the Shrewsbury country, and the second dating from much later times when Powys had retreated twenty miles further west and its earlier extent was partly forgotten.

On the whole, Professor Williams carefully avoids either too much emendation or too little. Many linguistic difficulties are still unsolved (as he would be the first to admit, for unlike Skene he does not pretend to understand everything), and most of them will probably never be solved at all; but in an edition of such extremely obscure poems there is bound to be much that is conjectural, and no doubt there are certain things which he will be inclined to reconsider later. Except for such details the text of this large and extremely important body of early Welsh literature has been established once for all. In making all these poems intelligible and in proving their authenticity Professor Williams has done a very great service to Celtic studies and British history. Now that this is completed we expect from him his long-awaited edition of the Gododdin poems, which though less informative are in some ways even more important historically than the present texts. May we suggest then when this book appears it should be published in English with a translation of the text, so that the many non-Welsh speaking historians, archaeologists, and scholars in England and abroad may be able to understand and appreciate it?

# The Römisch-germanische Kommission

by P. K. BAILLIE REYNOLDS

*Inspector of Ancient Monuments for England*

IT was nearly six years ago, in December 1929, that the Römisch-germanische Kommission of the Deutsches Archäologisches Institut celebrated its silver Jubilee. On that occasion Dr Édouard Meyer, in his opening address, reviewed the scope of the Commission's activities during the 25 years of its existence. The address, which is printed in the commemorative volume issued to mark the occasion,<sup>1</sup> laid some emphasis on the fact that the Commission's energies and achievements are not confined to the Roman period, but stretch beyond it in both directions, forward at least to Carolingian times, and backward into Prehistory. This wide extent of the Commission's interests is probably imperfectly understood by many students of antiquity in this country—if indeed there is not a tendency to confuse the Römisch-germanische Kommission with the Limeskommission—but it is by no means a new development. In fact it may be said that the 'Römisch' part of the title is an accident, for, as originally planned, the Commission was to be a body charged with the direction of all excavation in Germany, whether of prehistoric, Roman or post-Roman sites, and was intended to stand on an equal footing with the German Institutes in Rome and Athens. That it did not develop according to plan was due to 'zufällige persönliche Konstellationen'.<sup>2</sup>

In a pamphlet published in 1934 Dr Carl Schuchhardt, one of the few surviving original members of 1902, gives a brief account of the origin and development of the Commission which he helped to found.<sup>3</sup>

<sup>1</sup> *Fünfundzwanzig Jahre Römisch-Germanische Kommission*. Walter de Gruyter and Co., Berlin and Leipzig. 1930.

<sup>2</sup> Gerhart Rodenwaldt. *Archäologisches Institut des Deutschen Reiches, 1829-1929*. Walter de Gruyter. p. 43.

<sup>3</sup> Carl Schuchhardt. *Die Entstehungsgeschichte der römisch-germanischen Kommission des archäologischen Instituts des deutschen Reichs zu Frankfurt a.M.* Privately printed. Petzold-Druck., Syke, Bremen. 1934.



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He describes how opposition and political influence led to the curtailing of the original scheme, to the fixing of the status of the newly-founded body as a 'Commission' instead of as a 'Branch' of the German Archaeological Institute, and to the circumscribing of its activities by the limits reached by the Romans in their conquest of Germany. Within these limits however the Commission, with its headquarters at Frankfurt-on-Main, was authorized by Imperial decree to pursue the study not only of the Roman civilization itself, but also of the primitive cultures which preceded it; and since its institution it has also extended its investigations to the development of the civilization of the Romans in the hands of their successors. In effect it has extended its field beyond the limits of Germany, though in general confining itself within the boundaries of the Roman Empire and its 'spheres of influence'.

It is not unknown that there is now a movement in Germany to minimise the contributions made to German civilization by the classical and other cultures, and as a result there has been a suggestion to found a new body specially charged with the investigation of Prehistory and early history (*Vor- und Frühgeschichte*) exclusive of the Roman period and independent of the Commission with its Roman interests. Whether or not this would be to the advantage of research in these fields is a question which cannot profitably or properly be discussed in this country, and is a matter for German learning and the German State to decide; but it may be legitimate to remark that most of the knowledge of the Germanic peoples in the period immediately preceding the Roman epoch is derived from Roman sources, both literary and archaeological, and that the period of migration, without the background of the Roman Empire, is unintelligible. In this country it may be said that the pre-Roman, the Roman and the post-Roman are now regarded as phases of a continuous process of development, and are no longer separated into water-tight compartments: and the same should naturally hold true of Germany. Indeed it has been the peculiar task of the Römisch-germanische Kommission to deal with these three phases as an organic whole, and it cannot legitimately be accused of giving undue prominence to the Roman phase. This however is now being suggested, and it was largely to refute these suggestions that Dr Schuchhardt wrote his pamphlet, of which the most eloquent part is designedly the long list of publications dealing with *Vor- und Frühgeschichte* which have emanated from the Commission since its foundation in 1902, either as separate works or in the regular periodicals. The list is an impressive one, but doubtless it can be argued that it



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would have been more impressive still if the Commission had not had to devote a deal of its space to the Roman epoch. Be that as it may, it cannot be denied that in its treatment of *Vor- und Frühgeschichte* the Commission devotes as meticulous care to the preparation and presentation of its reports as it does to its studies in the Roman field. Whether or no it lacks in quantity, the quality of its work cannot be questioned.

Archaeologists in this country will watch with interest the outcome of this situation, and the decisions reached by the German authorities, for it must be borne in mind that in Germany archaeology is an affair of State, and has been since before 1871.

In view of this situation it will perhaps be of interest to record somewhat briefly what additions have been made by the *Römisch-germanische Kommission* during the last two or three years to its already notable list of publications, each of which is the fruit of many years of study.

In addition to the regular periodicals, which are noticed below, the Commission has in the past been responsible for the publication of six different series of monographs dealing with various divisions of its wide field of study. These works are issued only at irregular intervals.

The series entitled *Römisch-germanische Forschungen*, begun in 1927, has been increased during 1933-34 by the addition of volumes VI-IX. Of these VI deals with a purely Roman site, VII with a non-Roman Germanic burial with Roman material, and VIII and IX with pre-Roman subjects.<sup>4</sup> Vol. X, again on a Roman subject,<sup>5</sup> will probably have appeared before this review is in print, and XI and XII are advertised to appear shortly.<sup>6</sup>

Another series, entitled *Germanische Denkmäler der Völkerwanderungszeit*, of which the first volume appeared in 1931,<sup>7</sup> has been

<sup>4</sup> Vol. VI. F. Fremersdorf. 'Der römische Gutshof Köln-Müngersdorf'. 1933.  
Vol. VII. *Das Fürstengrab von Hassleben*. I. W. Schulz. 'Das Fürstengrab und das Grabfeld von Hassleben'. II. R. Zahn. 'Die Silberteller von Hassleben und Augst'. 1933.

Vol. VIII. K. Bittel. 'Die Kelten in Württemberg'. 1934.

Vol. IX. E. Sprockhoff. 'Die germanischen Vollgriffschwerter der jüngeren Bronzezeit'. 1934.

<sup>5</sup> Vol. X. R. Laur-Belart. 'Vindonissa, Lager und Vicus'. 1935.

<sup>6</sup> Vol. XI. W. Buttler. 'Die bandkeramische Ansiedlung von Köln-Lindenthal'.

Vol. XII. H. Mylius. 'Die römischen Heilthermen von Badenweiler'.

All this series is published by Walter de Gruyter & Co., Berlin.

<sup>7</sup> Vol. I. W. Veeck. 'Die Alamannen in Württemberg'.



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continued by the publication of vol. II in 1934. This volume, by Herr Zeiss, the Assistant Director of the Commission, is a good example of the wide field covered by the Commission's activities, since it deals with the finds from graves in the Visigothic kingdom of Spain.<sup>8</sup> Vol. III of this series is now advertised.<sup>9</sup>

The series of 'Materialien zur römisch-germanischen Keramik', which was begun in 1914 with Herr Oelmann's report on the pottery from Niederbieber, has been increased by the publication of volumes V and VI in 1933 and 1934.<sup>10</sup>

A new volume of the series, 'Germanische Denkmäler der Frühzeit' is said to be in the press.<sup>11</sup> Only 'Römische Grabmäler des Mosellandes und der angrenzenden Gebiete', and the catalogues of museum collections in west and south Germany, have received no additions since 1932.

In view of what has been said above, it is worth noting that of these six series only two deal solely with Roman matters, two deal with both Roman and non-Roman, and two with non-Roman only.

There are also one or two isolated works not forming part of any series, which have been produced by the Commission. A corpus of East Gallic coinage (*Die ostkeltischen Münzprägungen*) is in process of publication. Vol. I of this work<sup>12</sup> appeared in 1933, and vol. II is in active preparation. A third volume will complete the work. The late Dr Ritterling's *Beiträge zur Verwaltungs- und Heeresgeschichte von Gallien und Germanien* have also been published in conjunction with the Prussian Akademie der Wissenschaften.

The regular periodicals issued by the Commission are two in number, the Annual Report (*Bericht*), and the quarterly *Germania*.

The 'Bericht', though nominally annual, has not in fact appeared regularly every year, either before, during or after the war. The first number was in 1904, and the issues for 1932 and 1933 (published in the year following) are numbers 22 and 23. Each issue contains a brief

<sup>8</sup> Vol. II. H. Zeiss. 'Die Grabfunde aus dem spanischen Westgotenreich'. 1934.

<sup>9</sup> Vol. III. J. Werner. 'Münzdatierte austrasische Grabfunde'.

This series is also published by Walter de Gruyter & Co.

<sup>10</sup> Vol. V. A. Oxé. 'Arretinische Reliefgefäße vom Rhein'. 1933.

Vol. VI. A. Oxé. 'Frühgallische Reliefgefäße vom Rhein'. 1934.

This series is published by Joseph Baer & Co. Frankfurt a.M.

<sup>11</sup> R. von Uslar. 'Westgermanische Bodenfunde des 1. bis 3. Jahrhunderts n. Chr.'.

<sup>12</sup> R. Paulsen. *Die Münzprägungen der Boier*. H. Keller-A. Schroll & Co., Leipzig and Vienna.



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summary of the year's work of the Commission, under the headings of general, publications, excavations and visits. These activities will be noted more particularly below. The remainder of the volume is devoted to articles of considerable length and importance. Vol. 22 is an outstanding example of the width of the field covered by the Commission, and contains nothing at all on the Roman period. In an essay of 170 pages Herr Nestor, of Bucharest, gives a concise, but full account of the position of the study of Prehistory in Roumania, from the Neolithic period down to the beginnings of the penetration of Roman influences in the first century A.D. The remaining 70 pages are occupied by a study, on somewhat similar lines, of the Viking epoch by Herr Paulsen of Kiel. Both these articles are illustrated with line-drawings in the text, and with photographic plates, which for excellence of reproduction are hard to surpass—an observation which holds good for all the Commission's illustrations.

Vol. 23, by contrast, is devoted almost entirely to Roman material. Herr Koethe, of Bonn, contributes a comprehensive study of Gallic round and polygonal temples of the Empire. Besides a distribution-map (on which the rectangular temples of Gallic type are also shown) there is a complete inventory of all known instances from Gaul, Britain and the Rhine provinces, with plans of those the foundations of which have been traced, and the well authenticated examples are tabulated with brief notes as to the finds and dating material.<sup>13</sup> This article should be of service to investigators of Roman Britain, where more temples of this type may well be found.

On a not dissimilar subject is the second article, by Herr Schleiermacher of Freiburg, which is a study of certain Gallic and Germanic deities represented on monuments from the Rhine provinces. The author deals firstly with the gods depicted with a female consort, and secondly with Epona; and there is also a note on the Matronae type. There are a distribution-map of the representations of Epona, and three maps for the god with a female consort, according as the god has the attributes of Mercury, Mars or the 'Hammer-god'. Different markings on the maps distinguish the attributes of the consort of each type. The native deities of the provinces of the Roman Empire form a subject about which much yet remains to be learned, and this article has advanced the study by the collection of evidence, and by formulating certain conclusions which will be the basis of further investigations.

<sup>13</sup> This essay was to have been a volume of the 'Römisch-germanische Forschungen' series.



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The third article in this number is by Herr Reinecke of Munich, and is a complete list, again with distribution-map, of finds of 'Germanic' objects in Bavaria, which naturally include a high proportion of Roman manufactures, mostly found beyond the Limes.

Vol. 24, for 1934, has not been seen yet by the writer.

*Germania* was first issued in 1917, and has appeared regularly since. It is manifestly impossible within the compass of this review to mention individually all the articles which have appeared in it during the last two years, since each issue contains on an average nine articles.

Of the 72 contributions in the last 8 issues (July 1933 to April 1935) 14 are devoted to material of the palaeolithic and neolithic cultures, 11 to the Bronze and Iron Ages, 27 are on Roman subjects, and 14 deal with post-Roman times—the period of the Migrations and the Merovingian and Carolingian epochs. The remaining 6 are on miscellaneous topics. This proportion of roughly a third of the space being devoted to pre-Roman, Roman, and post-Roman subjects is typical of the policy which the Commission has followed for many years, and which is illustrated by its Directorate being composed of experts in these three phases of German culture.

It is worth notice that *Germania* for January 1934 contains Mr M. R. Hull's report (in German) of the 'Samian' potteries found at Colchester. This is a good illustration of the width of the Commission's interests, and it is remarkable that such a report should have appeared in Germany before the shorter notice of the site was given in 'Roman Britain in 1933' in the *Journal of Roman Studies* (vol. xxiv). Two other articles which may be noted by students of Roman Britain are that by Herr Oelmann in July 1933 on the Gallic temples (in this article the author maintains, *inter alia*, that the Wroxeter temple is of Gallic and not of Classical type<sup>14</sup>) and secondly the report in the issue for April 1934 on a 'Samian' pottery of the Trajan-Hadrian period at Aachen-Schönforst. Wares from this pottery may yet be identified in Britain.

Each number of *Germania* contains in addition to the longer articles short notices (*kleine Mitteilungen*) of recent finds, a section entitled 'Fundchronik', which is a record, arranged geographically, of finds relating to antiquity from the various districts within the province of the Commission. The finds of each district are given under the three headings of Prehistoric, Roman and post-Roman. There are also

<sup>14</sup> cf. Bushe-Fox, Second Report on the Excavations at Wroxeter, p. 2 ff, pl. 1, and Collingwood, *Archaeology of Roman Britain*, p. 139, fig. 35.

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reviews of recent publications, a bibliography, with list of contents, of periodicals on kindred subjects published in Germany and other countries, and a list of accessions to the Commission's Library at Frankfort, which show that the collection of books is kept well up to date. Indeed it is the best equipped Library of its kind in western Germany, and is steadily increasing, and outgrowing its accommodation. But in this and in other respects the Commission has suffered from the general financial stringency. The rebuilding of the library has had to be postponed, contributions to excavations have had to be curtailed, and the grants made to students to aid them in their research have also seriously diminished. In spite of this the 'Bericht' for 1932 and 1933 indicates that the Commission is doing its best with straitened means to carry on with its proper tasks in its own sphere.

The breadth of the Commission's interests is further exemplified by the welcome it accords to foreign scholars, who, no less than native students, are accommodated in its buildings at Frankfort, and whose visits are recorded with satisfaction in the 'Bericht'. An example of this welcome is afforded by the tour of the Lower Danube, organized by the Commission in the autumn of 1933, and reported in the 'Bericht' for that year. This assumed an international character, for in addition to Germany, England, France, the Netherlands, Switzerland, Austria, Hungary, Jugo-Slavia and Roumania were all represented. Such intercourse of scholars of all nations cannot but have the most beneficial effects both on the increase of the knowledge of the past, and on the relations of peoples in the present.

The activities of the Director, Herr Gerhard Bersu, and of the Assistant Director, Herr Hans Zeiss, in delivering lectures, attending meetings, visiting excavations, etc., occupy considerable space in the annual reports, and the other members are not less active. In spite of financial and other difficulties, the Commission may be said to be faithfully fulfilling its task of furthering the study of German civilization from earliest times to the end of the Roman epoch, and beyond, both within and without the limits assigned to it by its regulations.



## Notes and News

### A PRIMITIVE THRESHING-MACHINE (PLATES I-III)

In the Editorial Notes I have emphasized the importance of studying primitive customs of the living to enliven one's prehistoric researches. An admirable instance occurred recently in Cyprus. As I was strolling through the village of Ayios Amvrosios on the north coast, I came quite unexpectedly upon a tribulum, leaning against a tree. Next day I photographed it (PLATE I), and obtained particulars of the method of manufacture.

The tribulum is a wooden sledge set with flint teeth, and it is driven over the corn to thresh it (PLATE II). It is made by the village carpenter, who also makes the wooden parts of ploughs. He showed me how it was done. A number of wedge-shaped holes are made in the bottom of the sledge—there are 345 in the specimen illustrated—and into these flint flakes are knocked with an iron hammer. They are hit on one of the long sides, mainly near the bulbar end, bruising and battering the edge considerably. This was done in my presence. I also examined the condition of the flakes in tribulums which have been in use for a long time and removed some of them, including those here illustrated. The battered (lower) edge had been worn smooth by use, sharp excrescences having even been worn down and polished to a high degree. This doubtless is caused by the silica in the straw, over which the sledge is driven. The pressure exerted upon the flints by the weight of tribulum and driver, aided by a ballast load of stones, must be considerable.

I submitted specimens of the flakes to Dr Grahame Clark, who has most kindly drawn them for me (FIG. 1) and who observed yet another feature, namely, the existence of longitudinal striae (visible under a lens) upon the polished surface. Dr Clark thus summarizes the characteristics of the flakes :—

'They are primary flakes, with a bulb and platform intact, and they have the following secondary features :—

- (1) one edge sharp, the other with more or less battering
- (2) the battered edge only shows diffuse lustre of the type associated with friction against corn-stalks
- (3) this same edge is rubbed smooth by use
- (4) on the surface of the smoothed areas striae can be seen under a glass, running parallel with the line of the flake '.

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The archaeological importance of these facts will be obvious. If all these four secondary characteristics are present upon a flake, there can be no doubt about its use; they prove that agriculture was practised by the people in whose culture-stratum they are found. It is remarkable that no close examination of tribulum-flakes seems to have been made before. For if they are found during the excavation of a site they contribute a most important piece of knowledge about its culture. It is suggested that in future excavators should examine closely such flakes as they may find (after washing them) with a view to determining whether they were so used. The number lost must have been enormous; even in the specimen illustrated there are about 20 empty holes (I was only responsible myself for two); and no doubt the flakes need entirely replacing from time to time, after they are worn quite smooth. The flint is presumably obtained locally; it occurs near the village.

If the tribulum was used on a site, flakes from it should be found literally in thousands. The Cypriote threshing-floors are situated immediately outside the village, and those I saw were on bare smooth earth. At the time I was leaving the island (at the end of May) the harvest had only just begun in the Mesaoria, where it is earlier than on the north coast. I secured a photograph of some children playing at threshing (PLATE II), which illustrates the process quite well enough. The driver is seated on a special wooden structure, but ordinary chairs are also used. I also saw and photographed a tribulum on sale outside one of the shops in Nicosia (PLATE III).

Enquiries from one or two 'flinting' friends have failed to produce any evidence of tribulum-flakes from Britain. It is hoped that the publication of this note may result in the re-examination of collections and the close scrutiny of future finds during excavations. Weathering and patination might, of course, obliterate or obscure some of the four criteria; but the presence of one battered edge would be suggestive, if it occurred constantly.

In his chapter on flint flakes and cores, Sir John Evans (*Ancient Stone Implements*, chapter XII, p. 256) has the following remarks:—

'There is, however, another cause why rude splinters of flint should accompany Roman remains, especially in the case of villas in country districts, for the *tribulum*, or threshing implement employed both by the Romans and other ancient civilized nations, was a 'sharp threshing instrument having teeth',<sup>1</sup> in most cases of flint. Varro<sup>2</sup> thus describes the *tribulum*:—"Id fit e tabula lapidibus aut ferro exasperata,

<sup>1</sup> Isaiah, chap. XLII, ver. 15.

<sup>2</sup> De Re Rust, lib. I, cap. 52.



PLATE I

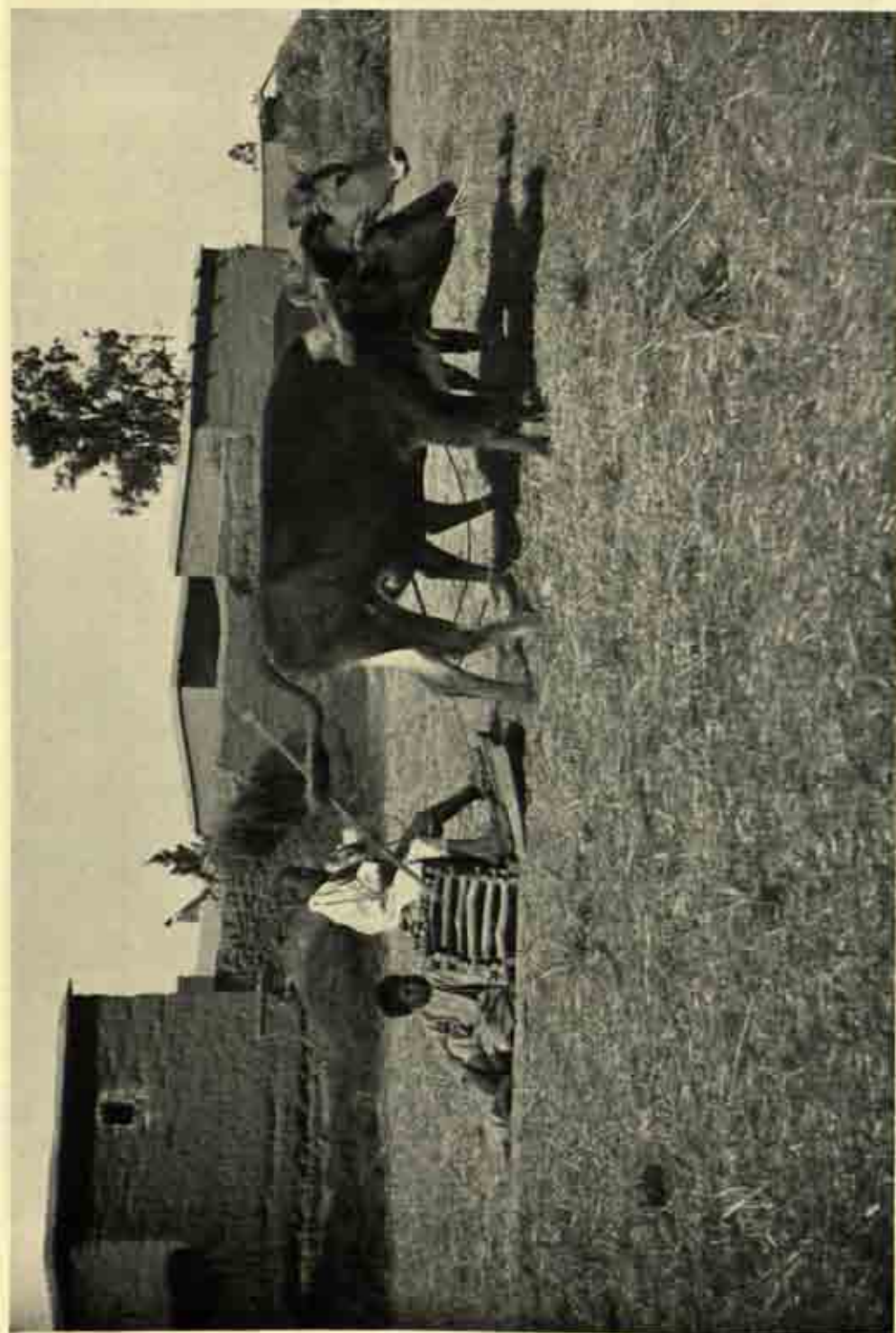


TRIBULUM IN THE VILLAGE OF AYIOS AMVROSIOS, CYPRUS (See p. 335)

Pl. O. G. S. Crawford

facing p. 336

PLATE II



TRIBULUM IN USE AT THE VILLAGE OF GEUNVELI, CYPRUS (See p. 330)  
Ph. O. G. & Crawford



PLATE III



TRIBULUM ON SALE OUTSIDE A SHOP IN NICOSIA, CYPRUS (See p. 330)  
Ph. O. G. S. Crawford

PLATE IV

IRON PLOUGH-COULTER,  
FROM THE WITCOMBE ROMAN VILLA,  
GLOUCESTERSHIRE (see p. 119)

*By permission of the British Museum*





PLATE V



MEGALITHIC CARVINGS (MAIN GROUP), ST. PIAT, LE HERCEAU (See p. 344)  
Ph. Stuart Piggott

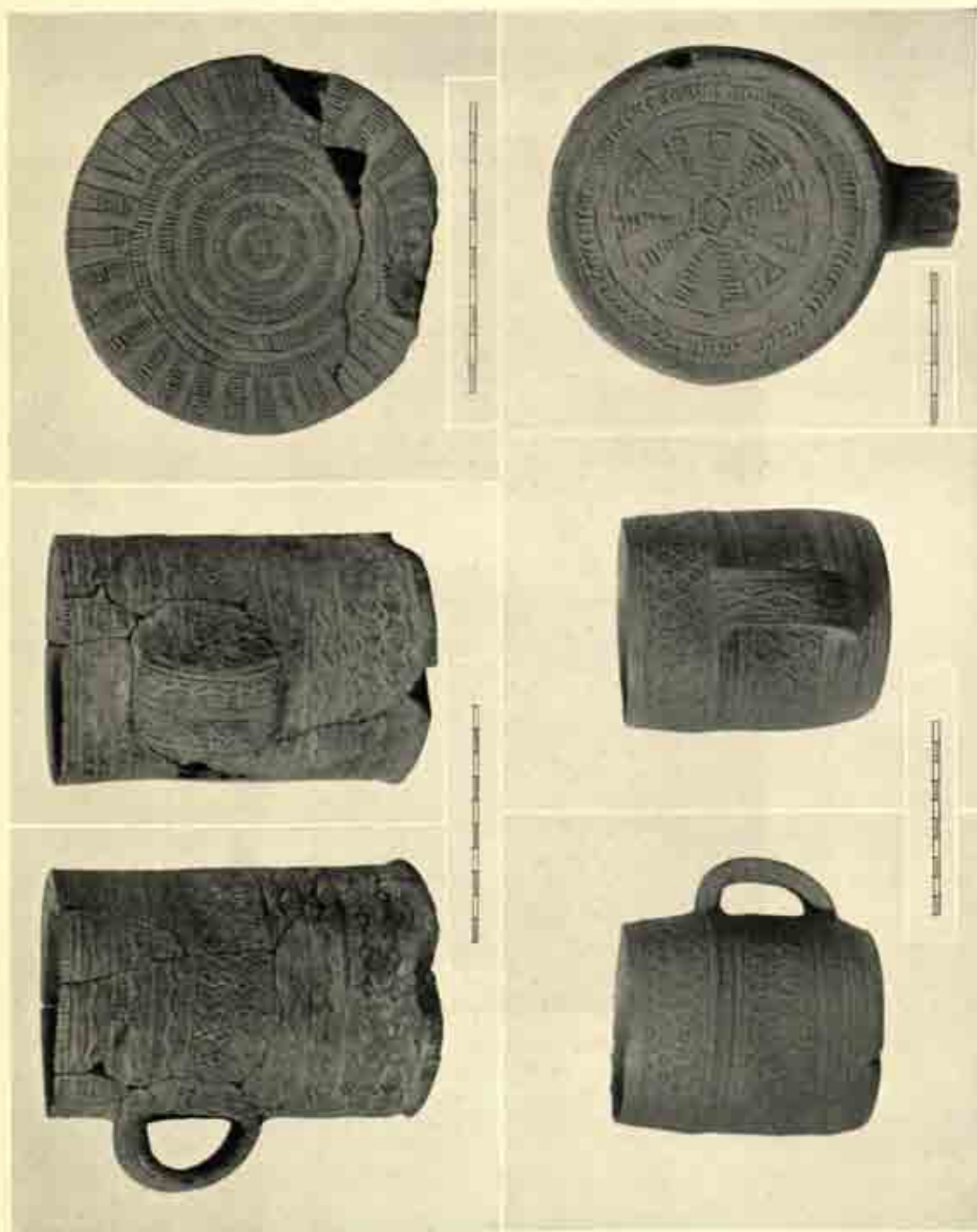
PLATE VI



MEGALITHIC CARVING, ST. PIAT, LE BERCHEAU (See p. 344).  
Ph. Stuart Pigott.



PLATE VII



HANDLED BEAKERS FROM DOTTISHAM (ABOVE) AND FORDHAM (BELOW) SHOWING DECORATION ON BASE. (See p. 248). Scales in centimetres.  
University Museum of Archaeology and Ethnology, Cambridge





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quae imposito auriga aut pondere grandi trahitur jumentis junctis ut discutiat e spica grana". Another form of the instrument was called *traha* or *trahea*. In the East, in Northern Africa, Madeira, Teneriffe, and probably other parts of the world, threshing implements, which no doubt closely resemble the original *tribula*, are still in use. The name is still preserved in the Spanish *trilla* and the Portuguese *trilho*, but survives, metaphorically alone, in our English *tribulation*. Drawings of various *tribula* have been given by different travellers,<sup>2</sup> and the implements themselves from different countries may be seen in the Christy Collection and in the

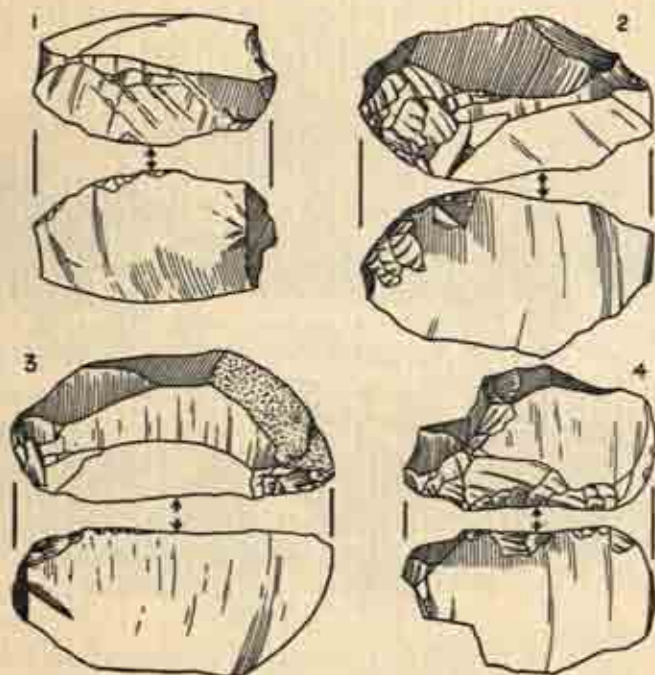


FIG. 1. CYPRIOTE TRIBULUM-FLAKES SHOWING BATTERED AND POLISHED EDGES, INDICATED BY ARROWS (See page 335)

Blackmore Museum [Salisbury]. They are flat sledges of wood, five to six feet in length, and two or three in breadth, the under side pitted with a number of square or lozenge-shaped holes, mortised a little distance into the wood, and having in each hole a flake or splinter of stone. In those from Madeira this is a volcanic rock, but in that from Aleppo—preserved in the Christy Collection, and shown in fig. 194—each flake is of cherty flint, and has been artificially shaped. Occasionally there are a few projecting ribs or runners of iron along part of the machine, but in most instances the whole of the armature is of stone'.

<sup>2</sup> Smith's 'Dict. of Gk. and Rom. Ant'. s.v. Tribulum. Wilkinson's 'Anc. Egyptians', vol. II, p. 190: IV, 94.

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It only remains to add that, in addition to the British instances, there are nine tribulums amongst the agricultural exhibits in Monsieur Vayson de Pradenne's exhibition at the Trocadero (see page 257). Amongst them is one from Tunisia (1878) with iron guides and rough bits of flint only, not flakes, and another, from an unknown provenance, set with irregular squarish pointed lumps of some volcanic stone (? Madeira or Canaries). A minute specimen, about 14 inches by 6,



FIG. 2. THRESHING THE CORN IN ARMENIA  
From Layard's *Nineveh and Babylon*, 1853

set with small obsidian teeth, must have served a somewhat different purpose.

The following additional description of the use of the threshing-sledge in Armenia is taken from *Nineveh and Babylon*, by A. H. Layard (London, 1853, pp. 17-18):—

The abundant harvest had been gathered in, and the corn was now to be threshed and stored for the winter. The process adopted is simple, and nearly such as it was in patriarchal times. The children either drive horses round and round over the heaps, or standing upon a sledge stuck full of sharp flints on the under part, are drawn by oxen over the scattered sheaves. In no instance are the animals muzzled—'thou shalt not muzzle the ox when he treadeth out the corn': but they



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linger to pick up a scanty mouthful as they are urged on by the boys and young girls, to whom the duties of the threshing-floor are chiefly assigned. The grain is winnowed by the men and women, who throw the corn and straw together into the air with a wooden shovel, leaving the wind to carry away the chaff whilst the seed falls to the ground. The wheat is then raked into heaps and left on the threshing-floor until the tithe-gatherer has taken his portion. The straw is stored for the winter, as provender for the cattle. These processes of threshing and winnowing appear to have been used from the earliest time in Asia. Isaiah alludes to it when addressing the Jews (XXVIII, 27, 28; See translation by the Rev. John Jones):—  
'The dill is not threshed with the threshing-sledge, nor is the wheel of the wain made to roll over the cummin . . . Bread corn is threshed: but not for ever will he continue thus to thresh it: though he driveth along the wheels of his wain, and his horses, he will not bruise it to dust. The oxen and the young asses that till the ground shall eat clean provender, which hath been winnowed with the shovel and with the fan (XXX, 24). Behold, I have made thee a new sharp threshing wain (sledge) armed with pointed teeth (XLI, 15). Thou shalt winnow them, and the wind shall carry them away (XLI, 16)'.  
O.G.S.C.

### THE ROMAN VILLA AND THE HEAVY PLOUGH (PLATE IV)

Air-photography and excavation have between them told us a good deal about Romano-British peasant villages and the way in which their inhabitants farmed their land. But though it is common knowledge that the characteristically Roman feature of the British countryside was the villa-system, no one seems to have produced much tangible evidence so far about the method or methods of villa farming. As there are signs that this gap in the achievements of modern archaeology may shortly be in some measure filled up by new work, the present moment seems a suitable one for the publication of a highly relevant older discovery.

The famous Roman Villa at Great Witcombe near Gloucester was discovered and excavated in 1818. The work was in the main supervised for the landowner, Sir William Hicks, bart., by Samuel Lysons, then fresh from his great discoveries at Bignor and at the height of his fame, and the two papers he read on the subject to the Society of Antiquaries appeared as a single short monograph in *Archaeologia*, 1819, XIX, 178-183. The ambitious courtyard ground-plan, the fine suite of baths, the excellent mosaic pavements and the painted plaster of the walls all showed that here was a villa of (for this country) the first class. Lysons thought that 'such an edifice' must have been erected 'by one of the superior officers of the Roman government in Britain'; such guesses are no longer fashionable, but whoever the proprietor was, he must have been the owner of a big estate. Here if anywhere one would



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expect to find no mere peasant tillage, but real Romano-British 'high farming'. And among the 'great variety of utensils, etc.' recovered Lysons makes special mention of 'a ploughshare of iron weighing seven pounds and a half, which has been presented by Sir Wm. Hicks to the British Museum'.

Enquiries made in 1934 by Mr E. S. Applebaum have led to the identification of this 'ploughshare', which is now figured for the first time (PLATE IV). It was duly recorded in the Museum book of 'Donations' under 13 March 1819, but there was then no compulsory painting of all acquisitions with their registration-numbers: its identity was forgotten and it never received a label. Now there are no heavy examples of true ploughshares of Roman date in the British Museum, and indeed a 'ploughshare' weighing as much as  $7\frac{1}{2}$  lb. would in any case be likely to be not a true share but a coulter. The Museum only possesses two coulters, and of these, one, unregistered and with apparently no history (though till recently stored in the Mediaeval Gallery), is disqualified by weighing 11 lb. 1 oz. The other, which weighs almost exactly 7 lb. 8 oz. and seems always to have been kept among the Lysons donations, is beyond doubt the Witcombe specimen, and it has now been numbered and labelled accordingly. It is  $27\frac{1}{2}$  inches long.<sup>1</sup>

We therefore now know that the Witcombe villa estate kept ploughs fitted with heavy wrought-iron coulters. Save for what appears to be a heavy kind of true share, with slanting 'ears' and a long shank, found in a hypocaust at the Box villa in Wiltshire, and now in the Devizes Museum (*Catalogue*, no. 494a (p. 59), pl. xxxiii, 1), this seems to be the first recorded piece of archaeological evidence for connexion between the Romano-British villa and the heavy plough. And whereas the Box implement is apparently not paralleled elsewhere,<sup>2</sup> the Witcombe coulter is an excellent example of the class well-known in Britain from the six found in the two hoards of blacksmith's scrap at Silchester,<sup>3</sup> and the five from the great iron hoard found in 1854 at Great Chesterford.<sup>4</sup> This class has recently been discussed by Lt.-Col.

<sup>1</sup> The coulter is mentioned by Lt.-Col. Karslake in the paper noticed below (*Antiq. Journ.* xiii, 455) as without known provenance: its identification followed a few months after that paper's appearance.

<sup>2</sup> It is clearly not a coulter, as the passage cited from the Devizes *Catalogue* might seem to suggest by mentioning the Great Chesterford coulters in connexion with it.

<sup>3</sup> 1894, *Archaeologia* LII, 742; 1900, *ibid.* LVII, 247.

<sup>4</sup> *Arch. Journal*, xiii, 1 ff.



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J. B. P. Karslake, F.S.A.,<sup>6</sup> who shows it to be a characteristic fitting of the wheeled plough or *caruca*, apparently as described by Pliny (*N.H.* XVIII, 43).

Discussion of the many issues raised in his paper would be out of place here, but in any enquiry into agriculture in Western Europe before, during, and after the period of the Roman Empire, an important part is bound to be played by correlation between any known type of agrarian unit, such as the villa in southern Britain, and any known type of agricultural implement, such as the heavy plough, and the type of tillage it implies. In this case considerations of weight and size show that the type of tillage implied should be some sort of strip-ploughing, for a heavy plough must cut a long furrow so as to turn as seldom as possible, and some kind of strip-field arrangement inevitably follows. The Witcombe coulter therefore gives an important hint of how the arable of at least one sort of Roman villa in Britain was laid out. Exactly what kind of strip-system this was, and what historical inferences may legitimately be allowed, are questions for the expert. It may be noted that readers of Paul Leser's *Entstehung und Verbreitung des Pfluges* will be able to supplement Col. Karslake's list of similar continental coulters, and that the coulter from Bigbury Camp near Canterbury<sup>7</sup> may reasonably be added to the evidence he marshals in favour of the introduction of the heavy plough to Britain by the Belgic invaders of the 1st century B.C.<sup>7</sup> Provisionally at least that view seems now to hold the field. Then may our connexion between heavy plough and villa be taken as far back as the Belgic invasions? What stages lie between the little single-steading farm of the prehistoric Celt and the estate centred on 'such an edifice' as the Witcombe Villa? Where archaeology is confronted with such problems, even small single pieces of material evidence, like that here brought forward, are likely to have their uses.

CHRISTOPHER HAWKES.

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<sup>6</sup> *Antiq. Journ.*, Oct. 1933, XIII, 455-463.

<sup>7</sup> Where much ironwork of late pre-Roman date has been found (*Arch. Journ.* LIX, 212 ff.: coulter, pl. II, 4a; *ibid.* LXXXIX, 87 ff.) and where Mr Jessup's recent excavations have established the fact of a Belgic occupation in the century or so before the Roman conquest, supervening on one of the earlier Iron Age.

<sup>7</sup> A smaller implement, perhaps a native adaptation of the heavy 'Belgic' coulter, was found in the Glastonbury Lake-Village: Bulleid and Gray, *Glastonbury L.-V.*, II, 369, 384 (pl. LXI, I, 28), where others are mentioned from Ham Hill and the Lisnacrog-hera crannog.



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### MEGALITHIC ENGRAVINGS (PLATES V-VI)

The megalithic monument containing the engravings illustrated is known as Le Berceau and is one of a group of such structures near the hamlet of Changé in the commune of Saint-Piat, 17 km. NW of Chartres (Eure-et-Loire). It consists of six supporters arranged roughly on three sides of a square, with the opening to the west, and covered by a capstone now broken in two. Until 1817 at least it was still more or less buried in what must have been the remains of a covering mound.\* There is now no trace of this, nor is there any record of discoveries within the now gutted chamber.

PLATE V shows the main group of engravings, which are on the support forming the north side of the present entrance to the chamber. The technique is that of light 'pocking' of the stone, which appears to be a siliceous sandstone allied to our sarsen. The symbols represented include crosses, at least one hafted axe, zigzags, a curious 'plant' form, and 'bucklers'. Near the upper edge of the support adjacent to that bearing the main group of engravings is another 'buckler', the lower part flaked away (PLATE VI). As to scale, the large 'buckler' of the main group is exactly 4 inches high exclusive of 'horns', while the isolated 'buckler' in PLATE VI is 4.8 inches across between the 'handles'. Both photographs are necessarily oblique, in order to obtain the contrast of reflected light from the smooth surface of the stone.

The engravings obviously suggest comparison with those in the Breton megaliths, but it is difficult to find exact parallels. Hafted axes somewhat similar to that of Le Berceau occur at Mané er H'Roëk (*Corpus des signes gravés des monuments mégalithiques du Morbihan*, pl. 24) while the curious loop on the axe-haft is paralleled at Penhape (pl. 57) and the crosses at Mané Lud (pls. 44, 48). The 'bucklers' seem to represent the final degeneration of such forms as those of Ile Longue (pls. 65-66) with the crest of curved strokes reduced to two curved 'horns'. In Brittany something similar seems to have occurred at Pierre du Moustoir, where the crest is reduced to three or four strokes on either side (pl. 138), while the rectangular form of buckler (somewhat resembling that at Le Berceau) is found at Ile Longue itself (pl. 60).

A sketch of the engravings has been published by le Commandant Octobon in his *Statues-Menhirs, etc.*, pl. XLVIII, 1 and 5, and their existence recorded by M. Henri Bellanger in *Bulletin de la Société Préhistorique Française*, 1934, p. 327.

STUART PIGGOTT.

\* Information from M. Bellanger.



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### PLUVIAL AND GLACIAL CLIMATES

Perhaps it is not unjust to say that, at the moment, some archaeologists tend to write of the divisions of Quaternary time as if they were settled beyond debate, while most geologists decline to adopt this view. The following brief observations on the relation of past Pluvial to Glacial climates may therefore be of interest, since they are written in general terms from a geologist's point of view: they attempt to show why many members of that profession are unwilling, at the present juncture, to support with enthusiasm world-wide classifications and chronologies. In reducing a large amount of material to small compass I am indebted to Professor P. G. H. Boswell, Dr G. Simpson and Dr W. B. Wright for many helpful discussions and for valued criticisms and suggestions.

It has often been stated that an ice age must be world-wide in its effect. This is based largely on meteorological considerations which, whatever their functions in the cause of an ice age, must ultimately be a primary factor in the growth, maintenance and decay of land-ice. The existence of such ice certainly reduces to solid form a certain amount of the total moisture available in the atmosphere and hydrosphere. One of the objects of these notes is to discuss whether such abstraction and return of moisture will cause wetter or drier conditions near to and remote from the land-ice.

In the first place we are familiar with cold deserts in high latitudes, and with their existence in Quaternary times near the great masses of land-ice. The occurrence of pluvial conditions in these places does not then seem to be a glacial feature. There is, in fact, a notable deficiency of moisture above and near ice caps.

In the loess plains of northern France the occurrence of river and stream activity seems to have been intermittent and to have marked warm rather than cold phases of climate. We are familiar, also, in England with the rather loose description of gravel fans and terraces being formed 'during the melting of the ice sheets' in districts in which no land ice was present. The phrase expresses, nevertheless, the belief that more water was available, and greater river activity took place, with improvement of climate. This need not imply greater rainfall. Spring floods below the snow line in mountainous districts produce just such fans and terraces. We are wise if we proceed with great caution before we assume that such signs of active streams and rivers indicate pluvial conditions in regions adjacent to perennial snow and ice. They may, and probably do, indicate the melting of snow and ice, and



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of the superficial layers of ground ice. We need more striking evidence before we can assume a change of rainfall.

In short, non-pluvial conditions usually occur near land-ice, and apparent 'pluviation' in such districts may indicate nothing of the sort. This does not apply universally to ice- and snow-covered mountains in low latitudes.

Are pluvial conditions inter-glacial? First we must prove that the supposedly pluvial deposits are in fact inter-glacial. For this there is no surer evidence, with all its shortcomings, than the associated fauna and flora. Secondly, if we are assured that the deposits are inter-glacial, we have removed the attendant conditions of a glacial climate, which, though non-pluvial, produces deposits of pluvial aspect. Any proved inter-glacial formation of fluvatile origin is, then, an indicator of the activity of the river, which is now gaining its supply ultimately from rainfall. If the deposits are torrential or markedly coarser than those that the river is now forming, we may fairly assume that they indicate a heavier rainfall, provided the gradient has remained virtually the same. In some rivers this similarity has been proved, their drainage lines are unchanged, and the fauna gives the requisite indication of temperature. The older deposits are almost invariably much coarser than those now forming and may fairly be considered to indicate pluvial conditions in inter-glacial times.<sup>1</sup>

In brief, there is evidence in and near glaciated areas of non-pluvial cold periods and pluvial warm periods.

When we turn to regions remote from well known glacial centres our difficulties are increased. What criteria have we for coupling pluvial periods with glacial or interglacial periods? The method most used so far has been (1) to compare the number of pluvials with the number of Alpine glacial phases, and (2) to study the association of human implements. Even if we can convince ourselves that the numbers of pluvials and glacials agree, how do we know whether they are synchronous or interpolated?

I suggest that under the best of circumstances we can only know how many glacials or pluvials there were in certain regions, and that to accept one such chronology for the whole world in the present state of our knowledge is misleading. In the polar regions there is evidence, so far, only of a greater and earlier Quaternary glaciation and of a lesser which still exists, separated by a period during which the land-ice

<sup>1</sup> c.f. 'World Climate during the Quaternary Period', *Q.J. Meteor. Soc.*, 1934, LX, 426-478.



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retreated but did not disappear. We do not know whether the warm period of the north polar region was synchronous with retreat or glacial advance in the Antarctic.

In this country grave doubts exist in the minds of many of us as to the number of glacial episodes, the age of the first, and indeed what we should call the first. Recent work on the Somme must cause us to reorientate many of our old ideas.<sup>2</sup> Even the Alpine sequence does not remain unassailed, for Italian and other geologists are insistent that four glacial phases cannot be recognized in their mountains, though lowering of temperature rather than increase of precipitation seems to have caused glaciation.

An ice age, from a meteorological point of view, may be world-wide, but we have an abundance of evidence to show that the growth of land-ice, presumably as a result of that stimulus, and the migration and decay of its centres of dispersal was governed by regional conditions. Thus the three great ice sheets of North America were almost certainly no more than partly contemporary, and the glaciation of Greenland is older, and has certainly lasted longer than any of them.<sup>3</sup> We are daily learning more of similar conditions in this country and in Europe. How do these span a fourfold glacial sequence, and in spanning it what were the effects of each and all of them on the rainfall of the tropics? Judging by the remaining volume of ice we are now only about half-way out of the last ice age, or half-way into the next: how then can we look on present conditions as normal?

It seems that we should keep two factors entirely distinct: (1) local glacial maxima and minima, each with a history of its own, and (2) meteorological episodes, by means of intricate and constantly changing climatic belts and storm-tracks, each influencing the other, forming and dispersing ice sheets, deserts, or rain-belts. Whether the sequence established in a part of the Alps is a pure record of the meteorological episodes remains to be proved.

Turning to the use of human implements as time-pieces, we meet some doubtful factors: we know little of the time value of perspective when we look towards the older industries, though there is reason to suppose that they lasted longer than the younger. We do not know what allowance of time to attribute to dispersal of forms, and that

<sup>2</sup> H. Breuil and L. Kosłowski. *L'Anthropologie*, 1931, xli, 449-88; 1932, xlii, 27-47, 291-314.

<sup>3</sup> L. R. Wager, *Geol. Mag.*, April 1932, has indicated that Greenland was already glaciated in Tertiary times.



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dispersal was far from general or uniform. We are faced at the start with the doubtful relation in this country of the Chellean or early Lower Palaeolithic to a glacial phase, we are not sure which phase with relation to the Alpine sequence. Although implements are by far the best zonal indices, we have much to learn and undue confidence obscures the real issues.

Is a pluvial climate as significant a thing as some of us imagine it to be? Heavy rainfall is a far less unusual event than the growth of an ice-cap on high or low ground. If it is true to say that, at least in mountains, there has always been ice, surely it is a great deal more evident that there have always been districts of heavy rainfall. There are tropical rains now, but we do not attach glacial or interglacial significance to them, nor to the annual migrations of the rain-belts. When we find that there has been more rainfall in country now desert or rather dry, though well within the tropics, we are perhaps unduly overcome with a desire to correlate. Above all I would suggest that variation of rainfall may be regional without a world wide cause, especially in warm-temperate and tropical lands, and that a little more rain than usual may have disproportionally great effect on the ground. Similarly a slight change of climate, and the eviction of goats, camels, and certain agricultural methods would render large parts of the Sahara and its borders habitable again, as they were not so long ago. Moreover growth or failure of vegetation, presence or disappearance of standing water, may have reciprocal, if minor, effects on the atmosphere.<sup>4</sup> Such factors lessen the significance of pluvials in correlation, though their importance to the human species is, and always has been, enormous.

If we are to associate regional pluvial conditions with regional glacial oscillations, and both with an ultimate meteorological episode, we must have an accurate method of synchronization. It may be claimed for De Geer's glacial varve-clays that they keep one condition a constant, *i.e.* the presence of ice, either by latitude or altitude, and give us the all important factor of solar radiation, the meteorological glacial or interglacial, shorn of all complications of its effect on the ground. De Geer has himself pointed out many of the pitfalls into which the careless or ignorant may slip in attempting such work, but, even if all these are avoided, the personal factor in many forms plays a dominant part that is not wholly acceptable.

If we accept the results we should find ourselves in possession of a synchronized chart of the 'health' of glaciers from high latitudes to

<sup>4</sup> R. E. Moreau, *Journal of Ecology*, 1933, XXI, no. 2.



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low over a long term of years. Now attention has been directed in East Africa to supposedly synchronous oscillations of wet and dry sub-epochs in the plains, advance and retreat of the glaciers in the adjacent mountains.<sup>5</sup> Any measurements of silt accumulations due to rains in these parts must take account of more than one period of rain per year, as at the present day, but the glacial varve is recording at the same time ice melting as a result of solar radiation in particular, an annual oscillation with possible variation of a term of years. Nilsson has correctly pointed out, therefore, that the correlation of glacial and pluvial banded clays, even in the same ideal district, is not so simple as it appears at first sight.

The main result of Nilsson's valuable work there is to suggest that local glacial advances were accompanied by wet sub-epochs in the plains, retreat with dry periods; in fact the more precipitation the more snow on the mountains and rain on the plains. But in Western Europe the arid loess plains are associated with cold periods and growth of ice, running streams with warm fauna and glacial retreat. So one gathers that in cold-temperate lands and high latitudes the pluvial was interglacial; that in tropical lands pluvial and glacial increase go together. Varves should synchronize the glacials of high and low latitudes: in fact they seem to record the retreat of glaciers in high latitudes (interglacials) and, as Nilsson has shown, the glacial (or pluvial) of low latitudes.

It will be realized, therefore, that long distance correlation is no easy matter and that glaciation may be dissimilar in cause and effect in high and low latitudes. How far are warm-temperate regions an almost inextricable mixture of these two, and is not the type-area of the 'standard' Alpine chronology within or near that belt? For these and similar reasons one feels that Quaternary climate was flexible regionally, in altitude and in latitude, in temperature and precipitation. Behind the local manifestations of climatic change lie, presumably, certain essentially meteorological factors such as Dr Simpson's theory demands, but it is probably incorrect to suppose that they would give identical and synchronous climatic changes over the surface of the world. Centres of glaciation had their own maxima even in this country: so also did regions of rain and desert wax, wane, and migrate regionally. It remains to be shown what essential connexion there is between rainfall and glaciation, local or world-wide.

K. S. SANDFORD.

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<sup>5</sup> E. Nilsson, *Geografiska Annaler*, 1931, XIII, 249-348.



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### HANDLED BEAKERS (PLATE VII)

Handled beakers as a class have been discussed by Sir Cyril Fox (*Arch. Camb.* LXXX, 1). The purpose of the present note is to call attention to certain features of form and decoration as exemplified by two vessels in the Museum of Archaeology and Ethnology at Cambridge. It seems that a dual division within the class should be made between handled beakers such as those illustrated, whose simple, mug-like forms have no connexion with the A C types with which they are culturally allied, and beakers of vaguely A C forms with handles attached.

The distinguishing features of the beakers illustrated are the cylindrical form, the massive strap-handles and the curiously decorated base, ornamented with circles and radial lines. The Bottisham beaker in particular suggests a non-ceramic form, and it seems probable that such vessels are copies in clay of wooden mugs turned from a log on a lathe. The beakers have a decidedly 'wooden' appearance, and the type of handle is precisely that which could most easily be carved from the solid projection left for this purpose in the turning. Moreover, such an origin for the type would explain the decoration of the base, the concentric circles being a reminiscence of the growth-rings visible in the transverse section of a log; the radial lines, the medullary rays and the cracks which usually form along them.

The existence of some form of lathe (probably a pole-lathe) in the Early Bronze Age has to be assumed to account for the turned shale and amber cups (cf. R. S. Newall in *Wilts. Arch. Mag.* XLIV, III-117). As Mr Newall pointed out, the use of amber suggests a Scandinavian origin and he instances wooden cups from Schleswig and elsewhere. The Nordic connexions of the A C Beaker culture, first pointed out by Childe (*Danube in Prehistory*, 200-201) has been emphasized by Clark (*ANTIQUITY*, 1931, v, 426) and if the origin suggested above for our handled beakers be correct, we have an important link between the turned cups and the A C complex.

STUART PIGGOTT.

### THE IRON SPEAR OF BUHEN

In reading the review of the second edition of Mr A. Lucas' very valuable *Ancient Egyptian Materials and Industries*, in *ANTIQUITY* for June, I note a passage (p. 238) on iron in Egypt which your reviewer quotes in inverted commas. Mr Lucas states of the iron spearhead which I found at Buhen, opposite Wady Halfa, in 1910, that 'more evidence must be adduced before the date assigned to this object can



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be accepted, especially as it is practically identical with spearheads used until not many years ago in the same locality'.

Now if this last sentence has any meaning it must mean that Mr Lucas suspects that the tomb in question was opened by some Sudanese warrior in comparatively recent years, perhaps during the Dervish War, and that the said warrior was careless enough to leave his own spear in the tomb. Let me assure Mr Lucas and your readers that his hypothesis cannot be seriously considered for a moment. The tomb, which I excavated with the greatest care, had not been opened for many centuries, if ever, after it was built. The evidence, which is given in detail in the publication of *Buhen*,\* while not positively excluding the 18th Dynasty is far more in favour of the 12th. It should be remembered that the 2nd Cataract is on the edge of the Sudan, which authorities as great as the late Prof. Gowland have held to be one of the earliest centres of ironworking. It seems not at all impossible that this weapon might have been obtained in commerce or war by a frontier garrison living several hundred miles south of the unwarlike capital of Egypt.

DAVID RANDALL MACIVER.

Mr A. LUCAS writes :—

As, at the time of writing, I am away from note books and works of reference, I am unable adequately to deal with Dr Randall MacIver's suggestion that the primitive iron-smelting in the Sudan may be of very early date [12th Dynasty, about 2000 B.C., or 18th Dynasty, about 1580 B.C. to 1350 B.C.]. This suggestion is supported by a reference to the late Professor W. Gowland, F.R.S., an eminent metallurgist and a writer on ancient metallurgical methods. In my opinion there is neither evidence nor probability for any such early date and it is now generally accepted that the production of iron from its ores originated in Western Asia and that the Sudanese iron-smelting is comparatively late. Since Professor Gowland wrote, much fresh information concerning early metallurgy has been obtained.

[The controversy centres round a definite object—the iron spear of Buhen—either this was found with 12th or 18th dynasty associations, or it was not. Surely it should be possible to prove or disprove this simple point? The exact typological analogies of the spearhead, its mineral condition, etc., are important, but secondary. We are

\* University of Pennsylvania: Egyptian Department of the University Museum. Eckley B. Coxe, junior, Expedition to Nubia, vol. VII. BUHEN, by D. Randall MacIver and C. L. Woolley. 1911.

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prepared to give space to a further discussion of this matter. What really is required is a full illustrated description of all the early finds of iron objects in Egypt and Mesopotamia ; but for the moment we had better confine ourselves to Buhen. EDITOR.]

### EGYPTIAN FRAGMENTS (PLATES VIII-IX)

The accompanying photographs, for which thanks are due to Mr E. H. SAWYER of Cairo, show two unpublished sculptured slabs of limestone which were built in the 11th century A.D. into the wall of a staircase inside the Bab el Futtûh, one of the most important gates on the north of Cairo. The dimensions of both blocks are approximately 24 inches by 18 inches ; they are apparently fragments of wall-reliefs from ancient Egyptian tomb chapels, and may be assigned by their style to the Old Kingdom.

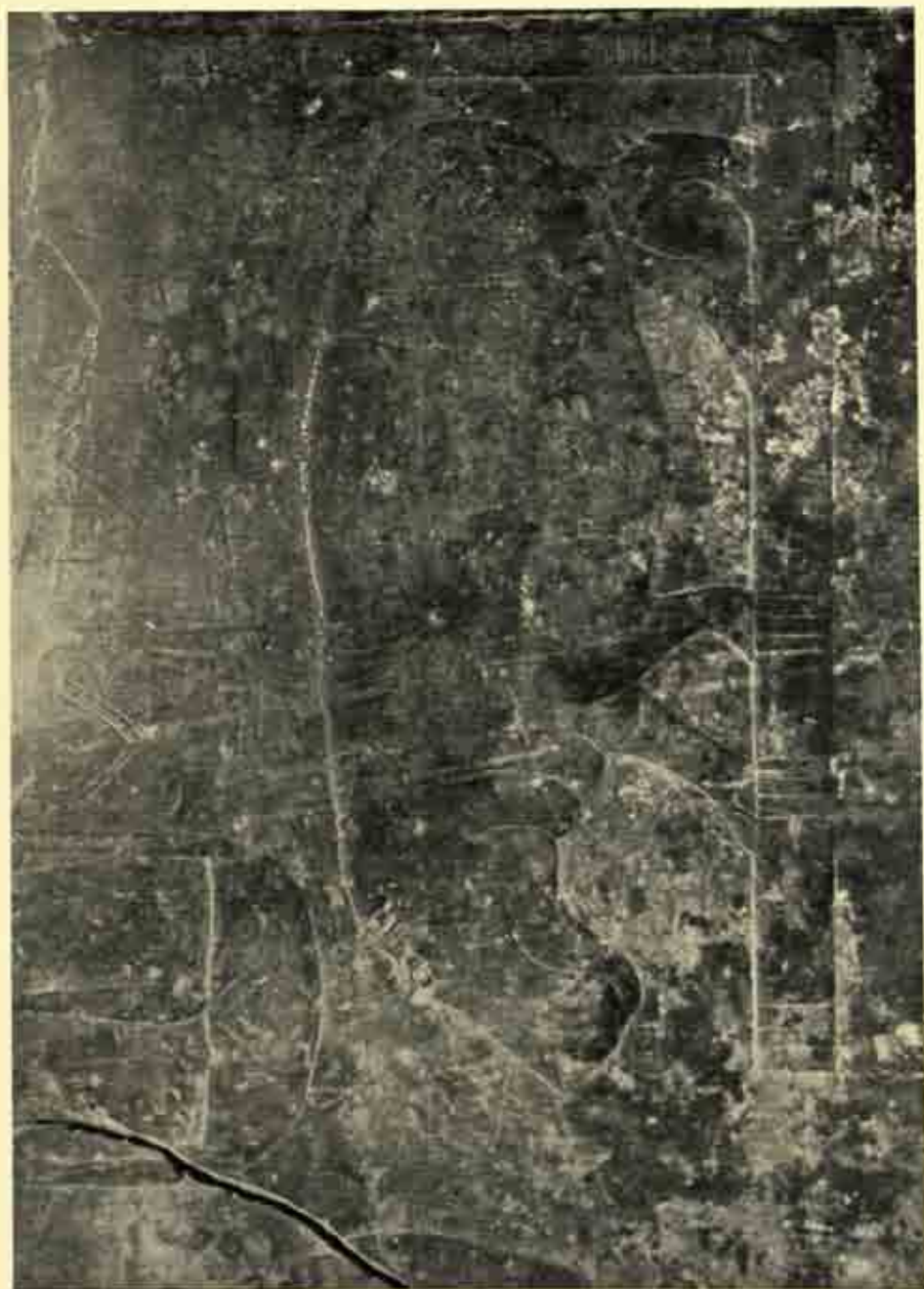
The relief (PLATE VIII) of the hippopotamus is a fine piece of work, perhaps the best portrait of this animal that has come down to us from the Old Kingdom. It is in very low relief, and for that reason photographs badly ; it should be dated probably to the 5th or 6th Dynasty. Above is the hieroglyph heb, 'festival', and a number of unintelligible scratches. Similar representations of a walking hippopotamus are not uncommon in the Old Kingdom,<sup>1</sup> but almost invariably they have a background of water (indicated by parallel wavy lines) and river plants, or are surrounded by fishes, in a scene of fishing or hunting in the marshes. The sudden cessation of the block-pattern border may indicate that the relief is unfinished, but the presence of a hieroglyph immediately above the figure seems to preclude the possibility that it was to form part of the familiar river-hunt scene which was a favourite theme of the Old Kingdom artists. In view of these peculiarities, it may be that the slab was a sculptor's trial-piece, and was never intended to form part of a tomb-relief. The sculpture has lost its original colouring, but being built into the wall at floor level, has escaped injury at the hands of visitors.

The other relief (PLATE IX) is less well cut, and contains portions of two scenes, divided by a thick line. The upper scene shows short-horned cattle, which have been artificially fattened for the slaughter-house, walking in single file as offerings for the owner of the tomb. They are presumably being driven by a herdsman at the back. There

<sup>1</sup> Von Bissing, *Die Mastaba des Gem-ni-kai*, bd. 1, pl. iv. N. de G. Davies, *Rock Tombs of Deir al Gebrawi*, part II, pl. iv.



PLATE VIII



SCULPTURED SLAB OF A HIPPOGRIFFUS, DAK EL FUTTUH GATE, CAIRO (See p. 350)  
Pl. E. H. Sawyer

PLATE IX



SCULPTURED SLAB OF CATTLE AND OF MEN CARRYING OFFERINGS, HAB EL FUTUH GATE, CAIRO (See p. 350)  
PH. E. H. Sowyer



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are two registers, divided by a thin groundline; the animals on the top line have only their legs remaining. The hieroglyphic group between the first and second animals of this row is illegible in the photograph. The lower scene depicts three men bringing offerings in shallow bowls or baskets; the contents of the bowls are stated above them to be km'i, and seem to be round berries or fruits of some kind. The word is not found elsewhere. Below the arms of the third bearer of offerings there was a further inscription, of which only n and the tops of two other signs are visible. The hindermost figure differs from the rest; he wears a diagonal linen band passing over the left shoulder and beneath the right arm. In the Old and Middle Kingdoms this is sometimes a mark of noble birth, and is also worn, both then and later, by the kher-heb or lector priest as a badge of distinction. The present figure is probably a lector priest; he carries in his left hand a similar linen strip. Two of these cloths are often carried in the reliefs by men in the offering procession,<sup>2</sup> generally behind the bearer of incense; the man who brings them usually holds them outstretched before him, one in each hand. The offerer may here have another in his right hand, which is lost. The word above the offering looks like ...it but is very indistinct. The red colour of parts of the relief is still preserved, though the surface has been much disfigured by scratches and Arabic graffiti; the stone was built into the wall up-side-down, about three feet from the floor.

Cairo contains a large number of such fragments, often in the most unexpected places, and the majority of them still unpublished. Saqqara is the most obvious source, being near at hand and abounding in good limestone which the later builders could usurp. It is unlikely that the exact provenance of any of these reliefs will be traced; the tombs from which they came must long ago have been plundered and destroyed.

M. S. DROWER.

[The Editors wish to thank Mr E. H. Sawyer for the great trouble he has taken to secure photographs of these interesting and hitherto unrecorded sculptures. It is to be hoped that amongst the thousands who take their cameras to Egypt each year some may be found who will photograph the other fragments. There could be no more interesting task, but in this as in all else, good results cannot be obtained without skill and taking trouble].

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<sup>2</sup> M. A. Murray. *Saqqara Mastabas*, vol. I., pl. vii.

## Recent Events

*The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.*

Sir Flinders Petrie writes :—

'The list of pyramids in the March number of *ANTIQUITY* (pp. 15-20), omits some published ones, both large and small. Add

7A Meydum, Queen, IV, 52 c. (*Meydum and Memphis III*).

49A Lahun, Queen, XII, 52 c. (*Lahun II*).

53A Mazghuneh, N. Amenemhat IV ? XII. } *Labyrinth and*

53B Mazghuneh, S. Sebeknefru ? XII, 107 c. } *Mazghuneh, XXXIX-XLIX.*

Page 14, line 33, for Teti, read Pepy I'.



Canon Goddard suggests that the queer expression 'stabbed by flat stones' (*ANTIQUITY*, IX, 154, quotation) should have been 'slabbed'; pointing out that the author of the quotation, in the passage immediately following, speaks of 'pyramids formed of slabs of rocks'.



A trawler has dredged up a Roman vase from a depth of 900 feet on the Porcupine Bank, 150 miles from the west coast of Ireland. On the bottom is a rudely scratched drawing of an animal, possibly a bear, and a graffito, possibly C PISCI FAGI. This all sounds very fishy, but the find is vouched for by the National Museum of Wales and published in the *Journal of Roman Studies* (1934, XXIV, 220-1, plate XXV).



It is good news that the ruined city of Rayy (Rhages) in Iran is being scientifically excavated by the University of Pennsylvania Museum (Are we to connect this shift of scene with the activities of the Iraq



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government?). The city was wrecked by the Mongols in 1220, soon after which it was visited by the Arab geographer Yakut, who has left a description of it. Subsequently it has been wrecked again by 'commercial excavations'. It has yielded many splendid 'art-specimens' and, during the latest excavations, some most interesting examples of 'fine chalcolithic pottery', akin to the well-known egg-shell ware of Susa and Persepolis, but with an individuality of its own. (*Ill. London News*, 22 June, 1935, pp. 1122-3, article by Mr Arthur Upham Pope).



In the 10th century Rayy was chief of the Jibal Province of the Eastern Caliphate, and celebrated for its magnificence throughout the East. It was the birth place of Harun-ar-Rashid, and was regarded as second only to Baghdad (G. le Strange, *The Lands of the Eastern Caliphate*, reprint of 1930, pp. 214 ff.). May we suggest that vertical air-photographs of its ruins would be of very great interest, as revealing the plan, if it should be in any way possible to secure them? The depredations of the treasure-seekers may not necessarily have obliterated this.



The bicentenary of the death of Thomas Hearne, the antiquary, has been celebrated by placing a memorial in his native parish church of White Waltham, Berks. We owe a great deal to this single-minded, if somewhat unsociable, man. He lived for learning, and some of his editions of manuscripts are still the only ones available. Archaeologists will remember him for his records of discoveries not otherwise known. (Several of these are incorporated on the map of Roman Britain). The tablet was unveiled by Dr Salter, F.B.A., whose address we hope will be published in full. (*Maidenhead Advertiser*, 5 June 1935).



Monsieur Aufrère has unearthed an interesting fact of archaeological history. It appears that Boucher de Perthes regarded himself as the 'continuator' of the work of a brilliant young fellow-citizen of Abbeville, Casimir Picard, who died at an early age about 1837. M. Picard recognized that certain objects found in peat (not gravel) were

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'en usage à l'époque où vivaient dans notre pays des espèces d'animaux ou perdues ou éloignées'. This is, as M. Aufrère says, the basic formula of prehistory.



Boucher de Perthes was 'continuateur énergique et persévérant, mais mal préparé et passablement gauche'. M. Picard's mantle fell upon these shoulders. 'J'ai continué les recherches commencées', said Boucher de Perthes, and to him is assigned a safe niche in the temple of fame, for all his gaucherie and unpreparedness. (*La mouvement scientifique à Abbeville dans la première moitié du XIX<sup>e</sup> siècle, et les origines de la préhistoire*, par Léon Aufrère ; Abbeville, 1935, 4 pages).



A nice instance of a kind of 'fossil tradition' is given in a Paris fashion-paper, which forecasts that the fashions of the immediate future will be influenced by the exhibition of Italian art, held this summer in Paris.



Dr Marinatos, Director of the Museum at Candia, reports that recent excavations in the Arkalochori district of Crete have resulted in interesting finds, including hundreds of copper Minoan axes and a number of enormous swords, stated to be the largest number of prehistoric swords yet found in Europe, a number of silver axes, and 27 gold double-axes with golden shafts.



Dr Marinatos says the most important find is a copper double-axe bearing three lines of hieroglyphic inscription composed of characters which now make their first appearance on the relics of Minoan civilization. The characters are similar to but not identical with those on the Phaistos disc. The finds belong to the golden age of Minoan art, about the sixteenth century B.C. They have been taken to Candia Museum for cleaning. (*The Times*, 29 June 1935).



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Sir Arthur Evans in an article on the discovery of a 'Villa of Dionysos' (*The Times*, 29 July, p. 15) refers to this hieroglyphic inscription, and is of opinion that while one or two new forms appear, 'nearly all the signs find close parallels in the Cretan series' and that it indicates an identical language, the inscription dating from about 1600 B.C. In the same article Sir Arthur gives some particulars of an interesting building discovered at Knossos, in the vineyard of the Villa Ariadne, headquarters of the British School.



A fine marble torso of Hadrian was found resting on a floor-level presenting a series of rich mosaics in some small rooms north and south of the peristyle, from which they were separated by a surrounding corridor. The principal room contained a mosaic medallion of Dionysos, of whom a statue has previously been found in the vineyard. Sir Arthur considers that the building deserves a full investigation and suggests the cooperation of the British Schools at Rome and Athens for this purpose.



A valuable find of gold treasure is reported from Bócsa, in the Hungarian plain, between the Tisza and the Danube. It consists of the contents of an Avar chieftain's grave of the seventh century, and includes a gold goblet 6 in. high. (*The Times*, 11 June 1935).



Mr RAINBIRD CLARKE writes :—

'To the list of English Flint-knapping industries given in *ANTIQUITY* 1935, IX, 40-41, may be added one recently discovered at Linghills Farm, Great Cressingham, Norfolk. The site is in the stack-yard a few feet northeast of the farm buildings. About 300 yards away is the Blackwater ford of the river Wissey crossed by the Smugglers' road which forms part of the Icknield Way in Norfolk. (O.S. 6 inch, 71 SE). Long double-backed flakes of black unpatinated flint are found a few inches below the surface amid the débris of a destroyed out-building. The form and preservation of the flakes and the alignment

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of the remains of this flint-knapper's workshop with the existing farm-buildings suggest a fairly recent date for the industry. This is probably an outlier of the gunflint industry at Brandon, only 9 miles to the ssw on the border of Norfolk and Suffolk. Sample products of this extinct workshop are now in Norwich Museum'.

o o o

Two prehistoric lance-heads were found recently on the bank of the Trent, near Attenborough, by a Beeston schoolboy, Douglas Stewart. They are not made of flint, but of quartzite, similar to the material of the pebbles occurring in the local gravels and sandstones. It is probable, therefore, that they were made in the neighbourhood, although it is understood that they are the first to be recorded of their type in the area. (*Beeston Gazette and Echo*, 29 June 1935).

o o o

The custom of affixing horns and skulls to buildings and trees is very widely spread. I observed sheep-skulls on garden-walls in Cyprus, and in a tall pine by the Kyrenia-Karavas road (about 2 or 3 miles west of Kyrenia) a couple of horns are firmly fixed at a considerable height above the ground. 'Ibex horns are fastened to the corners of houses by the inhabitants [of the Hadhramaut, in Southern Arabia], a custom undoubtedly of religious origin'.

The accompanying illustration taken in 1932 at Eshery, near Sukhum in Abkhasia (Caucasus) shows that this kind of religious practice at any rate is not yet extinct in the U.S.S.R. What connexion, if any, is there between these skulls and the modern European scare-crow?

o o o

The following advertisement appeared on the front page (personal column) of *The Times*, 1 August:—

THE HOLY GRAIL.—The newly discovered Cup will be on view August 1st and each weekday, including Bank Holiday, from 10 to 6 p.m., at Palestine and Bible Lands Exhibition, 9 Tufton Street, Westminster (near the Abbey). Admission 1s.

It is of course perfectly legitimate to use the relics of the past (however venerable) as a means of raising money, but we should be glad to be told exactly how the advertiser knows that the 'newly





SCARE-CROW IN FIELD AT ESHERY, ABKHASIA, U.S.S.R.  
Ph. O. G. S. Crawford





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discovered cup ' is the Holy Grail ; assuming that by that phrase he means not the Arthurian fiction, but the cup used by Jesus Christ at the Last Supper ? (See also the *Daily Mirror*, 1 August, where the cup, which is of glass, is so described, and is said to have been ' found by missionaries in a cave between Antioch and Hamath ').



A Committee has been appointed by the Minister of Education in Egypt to inquire into the illicit sale of antiquities. This has been prompted by the attention given in the local press to the sale of papyri, but it is pointed out by *The Times* correspondent that the complaint has no real foundation, for the Cairo Museum, to which all finds have to be submitted first, does not interest itself in papyri, which are eagerly sought for by European museums. (*The Times*, 25 May, p. 13).



An account of the discovery of the Temple of Jason near Paestum is given in *The Times* (13 July, p. 13) by Signori Carlo Scarfoglio. In April 1934 Dr Zanotti Bianco and Dr Zancani began excavations, working on the particulars given by Strabo, who states that the Temple lay 50 stadia (about 9 kilometres) from Paestum. They were rewarded by almost immediate success and found at the exact distance sufficient remains to indicate the site of the Temple. Excavations have proceeded under the official Archaeological Survey, supervised by Professor Maiuri, and the whole site has now been examined, the foundations and column bases of a temple nearly 40 metres long and 19 metres wide being disclosed. Near it was a smaller temple which can be dated *circa* 7th-6th centuries B.C., earlier than the great temple. Here was found a quantity of Corinthian and proto-Corinthian material, including a statuette of Hera nursing the Child, a witness to the dedication of the sanctuary.



In the course of an article written for *The Times* (31 May) the First Commissioner of Works, the Rt. Hon. W. Ormsby Gore, M.P., puts forward an important proposal for safeguarding that part of Avebury which is in private ownership. He says :—

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' If this supremely important archaeological area is to be adequately safeguarded, as it should be, a scheme under the Town and Country Planning Act is essential.

' To be successful such a scheme will require the good will and cooperation of the local government authorities in Wiltshire, of the actual proprietors of the land, of the Ancient Monuments Branch of the Office of Works, and of the various public bodies interested in the progress of British archaeology. Of all such schemes the Avebury scheme, in my opinion, has priority from an archaeological standpoint. The site is unique, not only in Britain but in Europe, and it is important that the work of conservation and scientific investigation should proceed without further delay '.

We hope the First Commissioner will pursue the Scheme which he has in mind with all the vigour possible.



The peculiarities of the road-system in the Lincoln District are discussed in *The Times*, 3 August, p. 11. It is claimed that they appear to record a well developed plan of settlements for Roman veterans. The evidence in the district of local roads branching off at right angles—not only from the Ermine Street but from other roads in the area—points to their having been laid out by one authority for a distinct purpose, and that the territory of the *colonia* of Lincoln was planned for 'small holdings'—in fact it constituted a scheme for settling time-expired soldiers on the land.



Reference was made in the March number (p. 101) of *ANTIQUITY* to excavations at Mapungubwe, in the northern Transvaal. Captain G. A. Gardner, Research Officer for the University of Pretoria, informs us that the third season's work on this site was begun last June and that a quantity of well-made pottery and articles of native manufacture have been found.



## Reviews

ENGLISH ROMANESQUE ARCHITECTURE AFTER THE CONQUEST. By  
A. W. CLAPHAM, F.S.A. *Oxford: the Clarendon Press, 1934. pp. xvi, 180, 44  
text-figures and 48 plates. 30s.*

The old habit of referring to the English architecture of the century after the Conquest as Norman was convenient and not altogether inaccurate. That architecture had its roots fixed in Norman soil, its chief patrons were men who, if not natives of the Norman duchy, were intimately acquainted with it, and, if in time the art assumed a definitely national character, its earliest models were Norman importations. At the same time it was open to other influences and, considered from a wider point of view, it forms an important province of that Romanesque art which, with common principles of construction but with varieties of form due to local conditions, prevailed throughout Europe at the time of the Norman Conquest. This fact has long been recognized by foreign archaeologists. To a Frenchman, Norman architecture is not the Romanesque architecture of a particular region, but the characteristic form of early Gothic which was developed in Normandy from Romanesque; and the title of Mr Clapham's book reminds us that the Conquest, far from putting an end to Romanesque traditions which had hitherto pursued a faltering and unprogressive course in England, brought English builders into close touch with the most vigorous and fertile schools of Romanesque art on the Continent.

The ground which Mr Clapham covers is more familiar and attractive than the difficult field which he entered so successfully in the volume to which this is the sequel. His lucid method of exposition, aided by a series of admirable illustrations, brought home to us the merits and defects of pre-Conquest Romanesque, examined by a fresh and independent judgment which, in arriving at conclusions not always in keeping with those of his predecessors and possibly still open to dispute, was always sane and cautious. His present subject affords less opportunity for adventure and discovery, but there was ample room for a general survey of the art of the period, combining discussion of plan and structure with analysis of detail, and taking into account the most recent work which has been done upon the architectural history of individual buildings and in other local and specialized departments of study. Not only does Mr Clapham's official position as Secretary to the Royal Commission on Historical Monuments (England) give him special opportunity for a thorough acquaintance with buildings of every type, but his authority is fortified by extensive knowledge of the contemporary architecture of France. His discernment of the comparative value of work done by other students is apparent upon every page, and upon certain points, especially upon the history of sculpture, he can speak with an authority second to none.

Full space is given to the variations of plan in the larger churches, the types of parish-church plan form the subject of a separate chapter, and another is devoted to centrally planned churches. In his treatment of the larger buildings Mr Clapham



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describes their component parts in order: piers, triforium, clearstory and vaults are followed by towers, west fronts, crypts, windows, doorways and porches. The progress of plan and design is traced in chapters dealing with Cluniac and Cistercian architecture and with the churches of the later part of the 12th century. The discussion of the smaller churches is succeeded by three concluding chapters on masonry and mouldings, ornament and sculpture, and internal decoration and fittings. In spite of extreme compression of a wealth of material, the book is eminently readable, and Mr Clapham deserves congratulation for the skill with which he uses individual examples to illustrate his general points without succumbing to the temptation of dwelling upon details.

No work upon English Romanesque can fail to pay special attention to its masterpiece, the cathedral church of Durham, a photograph of the nave arcade of which is an appropriate frontispiece to the book. The development of Romanesque art in England was remarkable not merely for the imposing size of the building which it produced, but for the precocious achievement which covered Durham Cathedral with ribbed vaulting, designed at an earlier date than any which is recorded in the case of other buildings and completed, after a series of successive experiments and improvements, within some forty years of the conception of the design. It cannot of course be maintained that so great and so perfect an example of Romanesque construction, containing within it the germs of those principles of which Gothic architecture was the logical consequence, was absolutely the earliest building of the kind. But its priority to other dated buildings of the period, first demonstrated by Dr John Bilson towards the end of the last century, is a fact now generally admitted even by foreign architectural scholars who questioned his conclusions for many years. The builders of Durham were at any rate in advance of their age in the quickness of intellect with which they met and solved problems of vaulting, and their experiments with the ribbed vault introduced them to the structural advantages of the pointed arch and to the employment of a system of abutment from whose ultimate form in their hands it is no long way to the emergence of the Gothic buttress-system.

The aesthetic effect of such great designs as that of Durham enters but little into Mr Clapham's scheme, and from a book so well illustrated the reader may perhaps be safely left to draw his own conclusions upon this point. Among Mr Clapham's *obiter dicta*, however, is the remark that the peculiar design of the nave arcades of Gloucester and Tewkesbury is more striking than admirable, the justice of which few will probably dispute. The genesis of the dwarfed triforium in these buildings, so far as previous models are concerned, is an insoluble problem; but it may be mentioned that Dr Bilson's theory, alluded to in a footnote, of its derivation from the small upper triforium above the vaulted tribune in the earlier portion of each church is borne out by the survival of the four-storey elevation thus postulated in the east wall of the transept at Tewkesbury. With such an arrangement before their eyes, in which the height of the ground arcades was reduced by the importance given to the vaulted stage above, it may well be that the builders of the naves laid exaggerated emphasis upon their principal arcades by the simple process of omitting the upper stage altogether.

From such defects of exaggeration and omission Mr Clapham's book is free, and its value is greatly enhanced by the author's sense of proportion in dealing with the wide variety of topics included in his survey. It is possible here and there to add some example of a particular feature to those cited. To the four small wheel-windows mentioned on p. 68 might be added the one in the west gable of St. James' at Bristol, and in the same context it might have been remarked that the large 13th century window at Byland to which allusion is made belongs to a specially Cistercian tradition which



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is exemplified in the west fronts of the abbey churches of Les Vaux-de-Cernay (Seine-et-Oise) and Ourscamp (Oise). Allusions to churches in France and Normandy are frequent. If we may insist upon a small point, Mr Clapham's rendering of French place-names is not always exact. Cérisy (pp. 8, 9, 12) appears without its acute accent, and Saône (p. 3) and Cîteaux (p. 71) without their circumflexes, while the 'l' in 'Clairvaux' (p. 74) is obsolete. Araines (p. 98) should be Airaines. Among English names we notice 'Mells' (p. 104) for Mellis. Talley Abbey (p. 84) and Windrush (p. 131) are assigned to Glamorgan and Oxfordshire respectively: the first is in Carmarthenshire, the second in Gloucestershire. Walter of Hereford (p. 2) should probably be Robert. Lasingham (p. 67), colonized by monks who had seceded from Whitby, became a cell, not of Whitby, but of St. Mary's at York. The *Doppelkirche* at Schwarzhindorf is referred to among castle-chapels (p. 112), but this fine church, which stands so conspicuously on the right bank of the Rhine opposite Bonn, was attached to a nunnery, and the castle-chapel at Nuremberg and, so far as we know, that at Eger, are still in existence.

These points, however, can be revised in the next edition of a work which in plan and execution is of outstanding merit. One historical point is worth noticing. Mr Clapham mentions the monks from Marmoutier chosen *qui operi preessent* in the building of Battle Abbey. Similar phrases are frequently found in such contexts: at Canterbury, for example, William of Sens after his accident issued his orders through the monk *qui operariis prefuit*. These phrases, like so many others, come straight from the Vulgate, and a reference to 3 Reg. v, 15 and other kindred passages will show that the offices thus described imply administrative rather than architectural functions, and indicate that the duties of the monks at Canterbury and Battle were those precisely laid down for the guidance of William of Wykeham in one of his letters of appointment as clerk of the king's works. We are inclined to think that the position of Geoffrey de Noiers at Lincoln and later that of Elias of Derham at Salisbury were of a like kind, and allowance must be made for the poetical rhetoric which has won for Noiers the reputation of architect and artist.

A. HAMILTON THOMPSON.

**PALEOLITHIC MAN AND THE NILE VALLEY IN UPPER AND MIDDLE EGYPT:** a study of the region during Pliocene and Pleistocene times. By K. S. SANDFORD. (University of Chicago Oriental Institute Publications, vol. 18). University of Chicago Press, Chicago; Cambridge University Press, 1934. pp. xxii, 131, with 39 plates and coloured map, and 25 text-figures. 31s 6d.

This is the third volume of the Prehistoric Survey of Egypt and Western Asia produced for Dr Sandford by the Oriental Institute of Chicago. In the work which led up to the first two he was associated with Mr W. J. Arkell, but in the production of this, which not only covers new ground, but serves as a summary of his researches in the Nile Valley, he stands alone. These researches have occupied him for a period of ten years and it is gratifying to be able to say that the results fully justify the time spent on them. The sequence of industries which is correlated with the Nile and its terraces is now more complete than in the case of any other known river and forms a fitting prelude to the long story of the civilizations which ultimately flourished on its banks.

The history of the river may be said to begin long before the advent of man with the retreat of the Pliocene Sea which extended far to the south between plateaus of limestone. This retreat is punctuated by a series of terraces ranging up to a height of 750 ft. One



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at 300 ft. above the present alluvium is arbitrarily taken as the beginning of the Quaternary, but there is still no trace of man. Terraces at 200 and 150 ft. are likewise barren of his remains. In the 100 ft. terrace he makes his appearance in the Chellean stage. This terrace also records the evolution of his handicraft to the Acheulean stage, but the culmination of the latter is only reached in the 50 ft. terrace. The analogy both in levels and industry to the terraces of the Lower Thames is startling and even extends to the inclusion in the 100 ft. terrace of a rude flake industry analogous to the Clactonian. There follow in Upper Egypt terraces at 30 and 15 ft., a level unrepresented in the Thames, the course of which at that stage lay below sea level. The order of succession of these terraces is deduced not from typology or height, but from the fact that the implements of the older occur as rolled specimens in the younger. It is thus absolute and independent of any assumption whatsoever.

The Mousterian gravels are of exceptional interest, the higher or 30 ft. terrace containing early Mousterian implements, the lower more advanced forms. They form however no simple sequence like the 100 ft. and 50 ft. terraces, but are separated by a period of low level during which the Nile cut well below its present course. The 15 ft. or Later Mousterian gravels of the wadis are definitely gravels of aggradation, which thicken Nilewards on a sloping rock-terrace descending below the level of the alluvium.

The lowering of base level which preceded this aggradation Dr Sandford suggests is due to a depression of the surface of the Mediterranean. Of this there is not much direct independent evidence anywhere in the Mediterranean basin, but the idea of a low level at about this date is no new one.

The wadi gravels deposited during the Older Paleolithic bear witness to abundant rainfall all along the Nile. The equivalent gravels of the Mousterian period are less certain in their development and more torrential in their character and possibly indicate the beginning of a change. With the advent of the Newer Paleolithic the lateral tributaries had entirely failed and a steppe climate had established itself. This coincided with the arrival of abundant sediment from the south, which formed a cone or fan of silt very similar to that of the present alluvium. This silt spread laterally into the mouths of the wadis, which being dry made no contribution to it. There are no intercalated locally derived sands or gravels at their mouths. The silt cone has a height of 100 ft. at Wadi Halfa, 60 ft. at Kom Ombo and 20 ft. at Luxor, north of which it probably descends beneath the present alluvium. No explanation as to why its gradient differs so markedly from that of the terraces and the recent alluvium seems to be forthcoming for the present. It was followed by a second period of degradation and gravels formed at an early stage of this degradation but contemporaneous with the silts of the south are found 20 ft. below the alluvium at Beni Suef near the Faiyum. Still later aggradation led to the formation of the present alluvium.

The progressive impoverishment of the rainfall began in Mousterian times and did not reach its climax until after the Neolithic. It is significant that the evidence shows no sign of interruption by recurrent pluvial periods, such as might be supposed either to accompany or alternate with the Ice Ages of Europe; but in this connexion it should be remembered that the deposits of the Nile during the two periods of degradation are inaccessible, and if one may venture for the moment to go further than the author in the matter of speculation, it is the periods of low ocean level which one has learnt in Europe to associate with glaciation.

In the matter of speculative correlation Dr Sandford is so restrained that his most daring flight is the suggestion that the stages of the Nile are controlled by sea-level and



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are equivalent to those of the Mediterranean described by De Lamothe and Depéret, the correlation running as follows:—

NILE			MEDITERRANEAN
300 ft.	Terrace	— — — —	Sicilian
200	"	— — — —	Milazzian
100	"	Chellean and Acheulean	Tyrrhenian
50	"	Late Acheulean	Monasterian
30	"	Mousterian	"

With regard to this correlation the following criticism may be offered here. The stages of Depéret have been found not to have the wide applications which he ascribed to them. They fail on the one hand, as shown by Gortian, where recent orogenic movements have been in operation and on the other in glaciated and other loaded districts which have been affected by isostatic depression; but there is every reason to believe that they hold in areas of prolonged stability and the horizontality of all the deposits of the Nile Valley from the Pliocene onwards indicates that it is such an area. No raised beaches have however, as far as the reviewer is aware, been described on the Mediterranean coast of Egypt. Perhaps Dr Sandford may have something to say on this in his promised volume on the Delta.

In the matter of typology of implements Dr Sandford displays an admirable caution. As regards the Older Paleolithic he seems fairly confident that he is dealing with similar material to that of the type area in western Europe, with the possible exception of the crude flake industry of the 100 ft. terrace, which he only tentatively compares with the Clactonian. The Middle Paleolithic is 'plainly of the "Mousterian" plan' and he finds definite Levalloisian types among the rather scarce implements of the 30 ft. terrace. The post-30 ft. core-and-flake technique he regards as very different from that of the 30 ft. terrace. The continuance of the Mousterian industry in Egypt after the 15 ft. terrace he considers to be established by surface finds of more advanced types than are found in that terrace. Similar advanced types are found in the south in the base of the Sebilian silts and in the late Mousterian terraces of the north where gravel formation continued to a later date than in the south. The use of the term Sebilian is admittedly a make-shift, the connotation of the term, which was established by Vignard at Kom Ombo, south of Luxor, being greater than is desirable. The Sebilian of the silts is plainly an industry of Late Paleolithic type, but lacks many of the features of the Aurignacian, for example the burin. It appears to be characterized by excessive 'backing' of many of the blades, resulting in the ultimate production of crescents, which suggest an approximation to Capsian microlithic industry. The transition to the Neolithic is no more apparent than elsewhere, the stratigraphical records of any passage to the Badarian, if it exists, being buried beneath the alluvium. No Mesolithic industries such as the Azilian or Campignian have as yet come to light.

On the whole the memoir bears internal evidence of the most painstaking and cautious work, and is remarkably free from dangerous speculation. Of its archaeological and geological importance there can be no doubt, and it is both readable and beautifully illustrated.

W. B. WRIGHT.

MINNS VOLUME: EURASIA SEPTENTRIONALIS ANTIQUA IX. *Helsinki*, 1934. (London agents, *Luzac*). pp. 410. 30s.

It was a happy thought of our Finnish colleague, Tallgren, to organize a *Festschrift* in honour of the great English scholar who has been the pioneer in revealing the archaeological



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riches of Russia to the western world. But it was a still happier thought to use as the vehicle the ninth annual volume of the periodical in which Tallgren himself has been carrying on Minns' work. A *Festschrift* is usually a place where important contributions to knowledge are buried in oblivion; for the number of libraries that can bear the expense of such volumes is very small, and so the archaeologist of the future, who will have to rely on these libraries, will remain in ignorance of their contents. The many valuable papers assembled as tributes to Professor Minns will escape this fate; the volume forms part of a series now long recognized as indispensable on the shelves of any respectable archaeological institution, and seeing it, every reader will desire to have a copy for reference in his own library.

The volume is thoroughly international. The contributors include Bulgarian, Finnish, German, Hungarian, Latvian, Roumanian, Russian and Swedish authorities. The articles touch on all regions in northern Eurasia from China to England, and on all periods from the Old Stone Age to Viking times. Yetts describes how about 125 B.C. the Chinese learned of a superior breed of horses in Iran and describes the efforts they made to secure for breeding purposes animals of this western stock, identifiable with the famous Nesacan steeds of antiquity; before this date the Chinese, like their nomad neighbours, had been content with a small horse, only slightly improved from the wild steppe horse of Przewalski. The Luristan bronzes form the subject of two articles: Arne draws attention to striking European parallels that go back as far as the 'Tumulus Bronze Age' and extend as far west as South Germany. Hančar examines in greater detail the agreements between Luristan and the Bronze and Iron Age cultures of the Caucasus. In the course of his study he gives a very useful classification of the relics distinctive of the several cultural groups on both sides of the Caucasus. A number of special agreements between Luristan and the North Caucasian Koban culture are plausibly attributed to the descent of the Cimmerians, who would have intruded from the north into an older province of purely Asiatic metallurgy. But Hančar's Cimmerians can hardly be those to whom Nestor attributes the Late Hallstatt culture which he describes in Roumania.

Earlier Caucasian antiquities are discussed by Professor Tallgren, who adds many interesting details and illustrations to the account of the dolmens he gave in *ANTIQUITY*, 1933, VII, 190-202. These magnificent monuments and the 'statue menhirs' of the same region must now receive due consideration in all discussions of megaliths whether in Denmark or India. Tallgren cites Åberg's proposed reduction of the date of Troy II in support of his own chronology for the Caucasian monuments (1800-1500 B.C.). But Bittel's publication of the Cappadocian parallels to Troy and the American excavations at the site have robbed this argument of its force. The question will certainly be solved eventually by excavations in Iran where Tepe Hissar already reveals many, still undated, points of contact with the Kuban culture.

Burkitt takes us back to a far earlier period: he describes and illustrates the palaeolithic female statuettes from France to Siberia and shows how they characterize the Asiatic component of the European 'Aurignacian' culture. At the other end of the time-scale Kendrick explains with masterly lucidity and simplicity the really very intricate processes adopted by Anglo-Saxons and Vikings for the decoration of iron weapons. Scholarly papers by Alföldi and Fettich help us to appreciate how little the nomad invaders from Asia contributed to the art and culture of Europe. Archaeologically they can only be recognized by the manner in which they diffused or blended various cultural elements—including in the case of the Avars many Christian symbols—which



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they had picked upon European soil. Wace issues a timely warning against accepting too lightly the theory that the Dimini culture of Greece denotes an invasion from the Ukraine of Transylvania; the new discoveries in Macedonia and the Peloponnese have made such a migration seem less likely than it did ten years ago. And Makarenko describes a new 'neolithic' culture on the steppes between the Black Sea and the Caspian which may be earlier than the familiar contracted burials under barrows (kurgans). The dead are buried in the extended position without any external monument above the grave. Pottery and metal are absent but knobbed mace-heads do not look early. Nerman gives a lively account of the Swedish colonies on the Baltic as illustrated by his recent excavations and goes on to discuss the rôle they played in the great Viking expansion to the southeast that linked northern Europe with the Arabian world. But an enumeration of all the leaves in this wreath of tribute would take up too much space. The examples rather arbitrarily selected must suffice to show its luxuriant wealth. V. G. CHILDE.

PROGRESS OF ARCHAEOLOGY. By STANLEY CASSON. *Bell*, 1934. pp. 111 and 24 plates. 6s.

It is necessary first of all to say that Mr Casson's book is substantially a reprint of a series of articles which appeared in *The Listener* during 1933; but with the same breath one ought to make it quite clear that *Progress of Archaeology* is not one of those unhappy attempts to make a book out of popular newspaper articles which had much better remained forgotten.

Most of us at one time or another find ourselves criticizing a book for something that it does not intend to be, and therefore the reader will be wise if, at the outset, he understands that Mr Casson had no intention of presenting a text-book of world-wide archaeology. His aim was rather to make a reasonably general survey of recent archaeological knowledge, a survey of the results obtained from sundry excavations undertaken in various parts of the world during the last twenty years, and therefore it is manifestly unfair to set his book side by side for comparison with other accounts of particular and detailed areas.

The preliminary chapter entitled 'What the Archaeologist wants to do' sets out a most depressing view of the origin of archaeology as a learned study in the years following the Industrial Revolution. In this, of course, there is some truth, and no one would deny that the acquisitive habits of the 'Captains of Industry' in making private collections of antiquities were unscientific and reprehensible to a degree—Mr O. G. S. Crawford once discussed that matter with thoroughness—but there are still some of us who like to think that Camden, Aubrey, and Stukeley, antiquarians though they were, were the fathers of English archaeology. We should also have been pleased to see the name of Pitt-Rivers mentioned in connexion with the establishment of serious archaeological method in Britain.

The author's plan is to consider nine regions where recent research has produced tangible results, and by this means he covers in broad outline Europe, Asia, Africa, and Central America. Each region has its own complex problems; some of them are treated more generously than others, but on the whole there will be little quarrel with Mr Casson's judgment. One outstanding feature, however, is bound to cause comment, and that is the very inadequate account of the Old Stone Age discoveries in Africa and Palestine.

Britain itself is considered in the province of Western Europe, and what Mr Casson does is to emphasize the Continental affinities of British archaeology rather than to give a



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detailed account of the various excavations that have made a correlation between Britain and the Continent possible: indeed, one of the most important, that on St. Catharine's Hill at Winchester, finds no mention at all. Another disappointing matter is that Mr Casson, who is one of the most enlightened and provocative writers on sculpture, should have dismissed the Saxon Rood of Romsey in one line, without even a hint of the traditions of the refugee Greek sculptors which may have influenced its conception.

One of the best chapters is that dealing with Greek lands, in which the author is a notable specialist. Recent work, he points out, has amplified and not revolutionized our knowledge which was already very full, and future research may well be directed towards the Hellenistic Colonial settlements.

Mr Casson's style is attractive. It never becomes a subjective background for the display of archaeological wares, and although there is a welcome absence of archaeological jargon, the author has wisely avoided any attempt to popularize 'what, by its nature, is a study unsuitable for popularization, in the accepted sense of that term'. The book can be cordially recommended to the student and general reader alike. Its illustrations are above the usual standard; it is well printed and produced at a reasonable price, and is altogether a work on which Mr Casson can be congratulated.

R. F. JESSUP.

**DIE FELSBILDER VON YAZILIKAYA.** By KURT BITTEL. (Istanbul *Forschungen*, herausgegeben von der Abteilung Istanbul des archäologischen Institutes des deutschen Reiches, 5). Bamberg, 1934. pp. 11 and 31 plates. 10 marks.

The fifth volume of *Forschungen* issued by the Constantinople Section of the German Archaeological Institute contains a complete series of direct photographs of the famous sculptures in natural rock-clefts near the capital of the Hittite Empire. Maps, plans, and views are added to give a clear idea of the situation of the monuments. The position and the lighting of the carvings makes photography exceedingly difficult. Museum photographs of the plaster casts, taken by Humann in 1889, are accordingly added for several figures. They afford a clearer insight into the details than the direct photographs and incidentally reveal the damage which the monument has suffered in the last forty years. The author has prefixed to his photographs an exhaustive bibliography and summarized the various conflicting attempts at interpretation and dating. He has refrained from adding yet another attempt, but has left the archaeological world his debtor for a complete, well-reproduced and inexpensive record of these intriguing scenes of Hittite deities and ritual.

V. G. CHILDE.

**PAPYRUSFUNDE UND PAPYRUSFORSCHUNG.** By KARL PREISENDANZ. Leipzig: Verlag Karl W. Hiersemann, 1933. pp. xvi, 372, 2 maps. 20 marks.

A history of any branch of classical studies is apt to become a bald recital of long-forgotten names and repetition of outmoded theories. Dr Preisendanz's book has none of these defects; partly because papyrology as an independent science is of such recent growth—the date of publication happily coincides with the 70th birthday of Prof. Wilcken, who has done so much to create and guide it—and partly because Dr Preisendanz has mastered in an unusual degree the art of combining readability with scientific accuracy and detail. His book is, in short, far away the best and fullest survey of papyrology which has appeared. At the same time it suffers from serious defects: the author in a disarming preface refuses to be bound by the 'strict definition of the term papyrology',



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but this does not altogether compensate for the lack of a logical plan which is specially serious in what is bound to become a standard book of reference. Thus, the very detailed opening chapters, on papyri of medieval Europe (papyrus was used by the Papal chancery until the middle of the 11th century), and on the papyri of Herculaneum, with the fascinating story of the famous *Macchina dei papiri* by which the labours of 150 years have induced the charred rolls to yield up their secrets, do not excuse the very scanty notice given to papyri in languages other than Greek and Latin. Within these limits, however, Dr Preisendanz has written a lively chronicle of the most important discoveries, from the *Charta Borgiana* in 1778 to the two sensational finds of recent years, the Chester Beatty Biblical papyri (the earliest of them written within a century and a half of the Crucifixion) and the Manichaean religious books now being painfully but triumphantly deciphered in Berlin. Half-way through his task Dr Preisendanz abandons a chronological for a territorial arrangement, which makes it rather difficult to find one's way about and leads to some duplication; though perhaps inevitable, the change in method might have been better managed. The census of collections of papyri which follows is rightly stated to be incomplete, for it even omits items noticed in the preceding text, and the bibliography of papyrology at the end is definitely a mistake, for it is neither even passably complete nor germane to the purpose of the book. An excellent index is a redeeming feature; and on the whole Dr Preisendanz is to be congratulated on the way in which he has carried out an exceedingly difficult task.

T. C. SKEAT.

PRÄHISTORISCHE FORSCHUNGEN IN KLEINASIEN (Istanbuler Forschungen, 6). By KURT BITTEL. Istanbul, 1934. pp. 145 and 22 plates. 13.50 marks.

Asia Minor constitutes a natural bridge between the Babylonian cradle of history and the Aegean and Central European worlds which remained illiterate much longer. And indeed the earliest Bronze Age of Central Europe (and therefore its whole prehistoric chronology) is linked through Troy with Anatolia rather than with the Mediterranean and Egypt or Syria. Only the delay in exploring the Anatolian hinterland has forced prehistorians to resort to indirect and ambiguous connexions through Crete with Egypt for dating the Trojan finds and with it the Early Bronze Age of Europe. Conflicting interpretations of the often obscure evidence have given rise to controversies and uncertainties of a disquieting nature. Bittel's well-documented and illustrated book helps to settle some of these doubts, and becomes indispensable to a comprehension of Aegean and Continental prehistory. He not only gives by far the most comprehensive survey of the Anatolian material that has appeared, embodying the results of current excavations down to the middle of 1934, but also succeeds in establishing a reasonably probable system for the arrangement of the material.

It is true that for the culture-sequence in Central Anatolia Bittel has to rely almost exclusively on the American excavations at Alishar Hüyük. But he himself assisted in the operations and can supplement and control the unsatisfactory published reports from his personal observations. By the use of this material Bittel can re-establish the chronological position of Troy, refuting the attempts to bring the second city as a whole down into the second millennium B.C. For at Alishar goblets of a form peculiar to Troy II appear substantially earlier than cuneiform tablets of the 'Cappadocian' type. The partition of the Trojan material itself is assisted by reference to Miss Lamb's work at Thermi as well as to the results of the Americans at Troy itself.

Bittel does not accept without reserve Frankfort's theory of a relatively homogeneous culture extending right across the plateau from the Aegean to Armenia and even northern



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Persia. The earliest pot-fabrics from Central Anatolia are indeed black or, more rarely, red, but, though the forms are anything but primitive, the very characteristic types that are so familiar from Troy I, Thermi and Yortan are missing. First, in the second period, mis-termed by the excavator Alishar I, do we meet cups with high handles and the other well-known types, and even then in a much less developed form than in the west. Bittel stresses the contrast—perhaps overstates it; for comparison with contemporary cultures elsewhere reveals how exceptional cups with handles really are. And jar-burials and metal types disclose already a cultural community with the West. Moreover, late in the period of Alishar I, the two-handled goblets of specifically Troadic type appear there. Subsequently the divergence between Eastern and Western Anatolia becomes still more pronounced. The 'Cappadocian' painted wares extend to Van and Malatia on the east, but on the west barely cross the steppe belt between the Halys and the Sakaria. A map is inserted to emphasize this point.

The text contains the documentation for the foregoing conclusions, including a list of 158 sites, a large map and photographs and drawings of pots, axes, bronzes. There follows an admirably cautious comparative chapter. Particularly deserving of mention is the evidence there adduced that in Bulgaria Anatolian culture appears in *tells* as intrusive, and overlying settlements with the usual Thracian graphite ware. It would have been helpful if the author had discussed more fully the connexions of Anatolia with the Orient and their geographical bases. Two great routes cross the plateau: besides the familiar route of the Baghdad railway to Babylonia across the North Syrian plain, a famous caravan route runs through the mountains across Azerbaijan to Central Asia. And so in prehistoric Anatolia we find already, beside obvious indications of contact with Mesopotamia, no less explicit and more intriguing hints of connexions with Tepe Hissar in North Persia and Anau in Turkestan (pins with double spiral heads, copper stamp-seals, beak-spouted vases and so on).

V. G. CHILDE.

### THE ARCHAEOLOGY OF COOK INLET, ALASKA. By FREDERICA DE LAGUNA.

*Published for the University Museum by the University of Pennsylvania Press, Philadelphia, 1934. pp. 263. 72 plates and 5 text-figures. Price not stated.*

This report is based on three seasons' excavations by Dr de Laguna from 1930 to 1933 and appears with commendable promptness. It was hoped that one result of the work would be to shed light on the problem of the precursors of the present inhabitants, the Athabaskan Indians. Dr J. Alden Mason had suggested that the earlier inhabitants possessed an Eskimo culture, but Dr Birkett Smith had argued that the population only became 'Eskimoised' in the course of time.

The material from the excavated shell mounds falls into four stages of an Eskimo culture older than that of the Athabaskan Indians in this region. The material from the earliest period is of purer Eskimo type than that of the later periods, but the author points out that finds from period I are scanty and that it is not safe to base conclusions upon the few specimens that have been found.

There is an exceedingly technical chapter dealing with skeletal remains, but none of the older bones were complete enough to be of any value in determining the physical type. The author is cautious enough to say that Dr Birkett Smith's theory that the first inhabitants south of the Yukon were not Eskimo, remains neither demonstrated nor refuted.

The student of European prehistory will be particularly interested in the chapter on rock paintings. Those from Bear Island have a style of their own and the painting of a



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pregnant woman recalls distinct parallels with the south and southeast Spanish palaeolithic art. Dr de Laguna is unable to shed much light on the identity of those who made these paintings, but suggests that the Bear Island group belong to the older midden culture, because the paintings occur at a convenient height above its level. Such a dating criterion sounds a little naive, though it may carry more conviction on the site.

The plates which are intended to show the sites and sections are, on the whole, a waste of space. The plates of finds, however, are extremely good and were presumably taken in a studio. This strengthens the idea which one gets on reading this book, that the expeditions were undermanned. The presence of a good photographer on the expedition would have been of great service.

One glaring omission from this otherwise comprehensive report is difficult to understand. There is no index, and the list of contents in the front of the volume does not in any way repair this omission. This makes an already slightly clumsy report difficult to use with any speed.

NORMAN C. COOK.

LE MONDE ÉGÉEN AVANT LES GRECS. By P. WALTZ. Paris: Armand Colin, 1934. pp. 206, and 13 illustrations. francs 10.50.

This small summary of Cretan and Mycenaean culture appears to be almost exclusively a compilation from existing books. It is not up to date and is so dogmatic as to the course of Cretan history that the reader might well imagine that it was drawn up from the personal records of the kings of Knossos. The finds at Ras Shamra, a French excavation that has been in progress for several years, seem to have made no impression on the author's reconstruction of prehistory, while his title presupposes that he has no clear views on the date at which the Greeks arrived on Aegean shores; indeed it begs the most important question of all.

After learning that 'dans les Cyclades les poteries mycéniennes font concurrence à la céramique crétoise' (p. 83), that 'Atarissias n'est sans doute que la transcription du nom d'Atrée le père d'Agamemnon' (p. 90), and that the signs of the Phaistos disk were 'gravés au poinçon' (p. 156) the reader will soon realize that the author is unaware of the bulk of recent controversy and of the details of the subject of which he gives what is meant to be the best-informed and the most up to date summary in the French language.

Needless to add, one of the few and bad illustrations turns out to be that fifth-rate fresco-painting inevitably known as 'La Parisienne'.

S. CASSON.

ANTIOCH ON THE ORONTES. I, The Excavations of 1932. Edited by GEORGE W. ELDERKIN. Princeton University Press and Oxford University Press, 1934. pp. 156, 17 plates (one in colour) and text-figures. 63s.

The excavations at Antioch sponsored and conducted by Princeton have proved fruitful. This very thorough publication is further embellished by a section on the new mosaics from Yakto, which add considerable knowledge to what we know of the painting and mosaic of the fifth century A.D.

The reviewer inevitably turns to the fine mosaic illustrated in colours which has been awarded to Worcester Art Museum as its share of the discoveries. It comes from a floor of the first century A.D. It forms one of a series of splendid panels. The whole floor has been removed and the panels distributed. The central scene of the Judgment of Paris goes to the Louvre (since the excavations are in collaboration with the French authorities); a panel of a satyr and Bacchante goes to Baltimore, and Princeton gets a



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fragmentary but very lovely 'Aphrodite and Adonis'. Thus a mosaic which might have perished *in situ*, or languished in some Syrian museum, now serves to elevate and instruct the periphery of the ancient world. One interesting fact that resulted from the removal of the panels was that it was found that they had been brought by their makers to the house from the studio in a finished state, and let in complete into the floor. This fact is of great importance to the study of mosaic.

The importance of the panels, of which the Symposium of Dionysos and Herakles is the finest as a piece of composition and execution, lies in the fact that they evidently reflect paintings almost certainly of the Hellenistic age, as is indicated by the Hellenistic formulae of the border. The Judgment of Paris is weaker in composition than the Symposium, but reflects a distinct tradition of painting.

The Yakto mosaics, published by M. Lassus, belong to the border-period of the fifth century and contain remarkable scenes. It would be hard to decide whether they were designed by and for a Christian or a Pagan. One part of the mosaics shows the nymph Arethusa, rising from her pool. The main and larger floor shows a strange character. In a central *tondo* is the bust of a woman, anticipating in manner and shape the medallions of Byzantine mosaic of a century later. She holds her right hand raised, and in her left a bouquet of roses. She is labelled in the field as Megalopsychia. She is clearly the dominating motif. The rest of the panel shows a complicated hunting-scene in which take part the following named characters: Meleager, Actaeon, Teiresias, Narcissus, Adonis and Hippolytus. M. Lassus suggests with reason that each of these is a typical instance of Aristotelian megalopsychy, interpreted in the later manner of moralizing Christians, though there is no hint of Christianity in the scene. But it must have been a strange householder who adorned his floors with such stern stuff. Nor would the classical scholar immediately tabulate this list of offenders as perfect instances of megalopsychy. Rather one thinks of Alcibiades and Themistocles. Into this moral conception there seems to have crept a new hint of a quality that was likely to lead to destruction, more especially of those who defied the gods. This is indeed the East reinterpreting Greek morals against a Semitic background. Aristotle would never have given such a list of instances such as this!

Round the scene is a border in which are represented many of the main topographical features of Antioch, with the names of each building attached. It will evidently be of use to excavators. But the contribution of this mosaic is mainly iconographic. The Megalopsychia is indeed a remarkable subject, rather charmingly rendered. The bunch of roses evades the interpreter as an adjunct, and it is remarkable that such a moral quality should be rendered as so amiable a lady. Indeed a minor mystery. We have yet much to learn about the background of fifth century Greco-Syrian art. Unlike the first century panels this scene must have been composed *in situ*.

The remainder of this book describes adequately and faithfully the various areas excavated and objects found. Apart from the mosaics there is not much of importance. One gets the impression that Antioch has been much mishandled by the passage of time.

S. CASSON.

THE CAMBRIDGE ANCIENT HISTORY. Vol. ix (The Roman Republic).  
Cambridge University Press, 1932. pp. xxxi, 1022 and 18 maps. 37s 6d.

This volume includes essays on Pontus by Professor Rostovtzeff; on the Jews by Dr Bevan (an admirable sketch of the Jewish background to the Gospel story); and on Parthia by Dr Tarn—a vivid description of this vigorous people: 'one gathers the



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impression that they thought all religions useful, none material; what mattered to a man was his horse, his bow and his own right arm'. Mr Stevenson writes on Provincial Government, Mr Sikes on Literature in the Age of Cicero, Professor Wight Duff on Ciceronian Society, Mrs Strong on the Art of the Roman Republic and Professor de Zulueta on the Development of Law under the Republic. The long story of the wars of the century is told, necessarily on a scale more informative than stimulating, by a series of writers.

Politics, however, are the essence of this period of history, and even the great generals (Caesar excepted) are best known to us at the times when they had sheathed their swords. It is the period of the Gracchi, Marius, Sulla, Pompey and Caesar, all of them helping to destroy the Republic and all of them, as we are so well reminded over and over again in this volume, unconsciously laying the foundations of the Empire. And we have here a vivid account of the political history of the last century of the Republic, from the Gracchi of Mr Last to the Caesar of Professor Adcock. Mr Last, indeed, narrates the political history of the years 133-67 B.C. His Gracchi are altogether intelligible. Tiberius wanted to relieve unemployment: he was no theoretical economist or agriculturalist. Gaius was honest in his desire to enfranchise the Italians. Sulla was efficient and businesslike; he was no mere reactionary—indeed, some of his measures have a distinctly 'popular' tinge. If Sulla failed to see the real problem in remodelling the Roman constitution, it was because he had not the lesson of Caesar's career to guide him. Only in one respect does Mr Last's historical judgment seem open to question. Would the senate of 123 B.C. really have preferred to be swamped (for all purposes) by the admission of 300 new members, rather than surrender one of their functions, the control of the public court? On the other hand it is hard to believe that there can be any more lucid or convincing description of the *senatus consultum ultimum* than Mr Last has written on pp. 82 ff.

The Ciceronian period is described by Dr Cary but Cicero has perhaps suffered by compression between the great figures who preceded and followed his brief dominance in Roman politics. And finally there is Professor Adcock's realist Caesar, a dictator who, as far as the evidence goes, did not aim at establishing a dynasty; no would-be Hellenistic monarch-god but the holder of a position 'no more royal than it was divine'.

This volume is in all respects up to the high standard of its predecessors. In one respect it marks a large advance on them. There is, in addition to the usual excellent notes and bibliography at the end of the book, a great deal of valuable annotation at the foot of each page. This helps to explain, and fully compensates for, the extra weight of a volume which contains not much less than half a million words.

J. P. V. D. BALSDON.

THE PLACE-NAMES OF SURREY. By J. E. B. GOVER, A. MAWER and F. M. STENTON in collaboration with ARTHUR BONNER. English Place-name Society, vol. XI. Cambridge University Press, 1934. pp. XLVI, 445 and 7 maps. 21s.

No one can fail to admire the patient industry and scholarship of the compilers who have amassed so much detailed information as is recorded in this volume. It is a quarry for all time for the historian, the philologist and the archaeologist. But many deductions have already been made by the editors themselves, and their analysis of the material is contained in the lengthy introduction and in the appendices. A more bulky volume than is usual in this series has resulted from the inclusion of some 50 pages of field and minor names.



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Readers of ANTIQUITY will be interested primarily in the archaeological information provided. The name 'Surrey' (cf. Suthrige in the Anglo-Saxon Chronicle) signifies the southern region, the second element *gr* in the name being the O.E. cognate of the German *gau*. The Saxon invaders approached by way of the Thames and settled on both its banks: those on the southern bank in time spreading over the territory which came to be known as Surrey. This settlement is still commemorated in the names Middlesex and Surrey, but it had ceased to have an independent existence by the time that written records began.

The invaders spread up, and first colonized along, the tributaries of the main stream, as in shown by the various names ending in -ing (e.g. Woking, Godalming, Dorking, Tooting) on the banks of the Wey, Mole, and Wandle.

The place-names in the south of the county and furthest from the Thames have affinities with those of the neighbouring counties of Kent and Sussex, and the spread of the settlement naturally was checked by the Weald. Life in the clearings in the Weald can be visualized from the names of the villages with the aid of the distribution-maps. One of these shows that *lēah* (modern -ley), signifying clearing, is distributed generally over the southern half of the county, but that *falod*, signifying a fold for animals, and *hyrst*, probably signifying a wooded height, since it means both hill and wood, are densely crowded on the Sussex border and away from the main river-system. *Denn*, signifying a woodland pasture for swine, is also found in the wealden part of the county.

Another map shows the distribution of *ceart*, a word especially interesting with regard to Surrey, and occurring only on sand or sandstone. It survives in the dialect of Surrey and Kent as *chart*, and denotes 'rough common overgrown with gorse, broom and bracken'.

That the southwestern corner of the county was colonized early by the Saxon invaders is indicated by a collection of names there which were given before the arrival of Christianity. These names are Willey, near Farnham, whose O.E. form (*to*) *Weo lēage* is a compound of *wig*, *weoh*, an idol, and *lēah*, a clearing; and Peper Harow whose second element is from the O.E. *hearg*, a heathen temple. It is also suggested that the neighbouring Thursley and Tuesley may have been the clearings where Thunor and Tiw were worshipped. Thunderfield Castle near Reigate may also indicate the open space where Thunor was worshipped.

An ingenious interpretation of place-names formed from animal-head names, contributed by Prof. Bruce Dickinson, has been included as Appendix I. His argument is founded on a passage by Bradley saying that the heads of animals were set up on poles to mark the place for public meetings: various English place-names such as Gateshead (goat's head), Swineshead, etc., may therefore indicate the site of the heathen sacrifice of a head. The thesis is intriguing, but not proven.

There are fewer camps in Surrey than in many other English counties. A few, however, are given in this volume, and the name of Crooksbury Hill near Farnham is probably derived from the British *cruc*, a hill, and the O.E. *beorg*, barrow. Hambledon comes from *hamel*, scarred, and *dūn*, hill. *Beorg* occurs also in 9 other place-names in Surrey (p. 341), and *dūn* still more often (p. 342). Anstiebury Camp (Woking) derives its name from *anstig*, a path for one, and *burh*.

There are two names referring to clapper bridges, one under Cranleigh, and also the Clappers (Chobham), which presumably indicates a bridge across the Hale Bourne.

There are very few place-names in Surrey which have pre-Roman elements. Penge



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is the only Celtic name in the strict sense, but Walton on Thames, Walworth and Wallington derive from *wealh*, the usual word for British.

Two interesting names connected with Roman times occur near Godstone. Stansted House derives from the Roman road which passes it, running from London towards East Grinstead: the adjacent Flower Farm is probably connected with O.E. *flōr*, although no tessellated pavement from a villa has yet been discovered (cf. Fawler, Oxon, whose name in 1205 was Fauflor, derived from *fāgan flōre* [spotted or dappled floor], and where a tessellated pavement was actually found in 1865).

Appendices 2 and 3 deal with the name Coldharbour and the element Friday in place-names, respectively. Coldharbour appears 307 times as a name for a farm or homestead, and occurs especially in southeastern England. It is met with 30 times in Kent, 26 in Sussex, and 18 in Surrey. The name has been traced back to the beginning of the 14th century, and it is here shown conclusively that the oldest forms dispose of the derivation from *col d'arbre*. The real meaning of the word is, however, still uncertain.

Various names are found in the south of the county which are obviously survivals of the now extinct iron industry of the Weald. Such are Abinger Hammer, Hammer Pond (Thursley), Furnace Bridge (Dunswold), Hammer Bridge (Leigh), Cinderhill Wood (Bletchingley), and Tumbling Bay (Alford), which name indicates an 'out-fall or pool into which the water fell'.

Other derivations interesting to the archaeologist as illustrating medieval customs and institutions are: Northbreache Manor (Ewhurst), which is traced to O.E. *braec*, a 'strip of (uncultivated) land taken from a forest by royal licence, for temporary cultivation'; Woolpit (Ewhurst), i.e. a pit for trapping wolves; Cotland Farm (Charlwood), a word recorded as early as c. 1150 and meaning 'the piece of arable land, about 5 acres, held along with his "cot" by the O.E. *cottar*'; Rose Farm (Oxted), where the name is derived from the rent (in 1345) being fixed at the yearly payment of a rose at the feast of the Nativity of S. John the Baptist.

Various curious corruptions ('folk-etymology') occur throughout the volume: perhaps the following are worth quoting here. St. Martha's Chapel, Guildford, was early granted to Newark Priory, which was dedicated to S. Thomas the *Martyr*. The form Momartre extra Gildeford appears in 1273 for this. Galley Wood (Cranleigh) is corrupted from the 1298 form (West-)gavelwode, the first element being the O.E. *gafol* (tax). The form Hendodd's Wood (Bletchingley) of 1761 has now become Hangdog Wood. The 1548 form Chertelease (Thursley), which is derived from *ceart* (see above) and *lae* (pasture), appears today under the name of Chocolates. WILFRID BONSER.

CAMBRIDGE ANCIENT HISTORY. Vol. IV of Plates. Prepared by C. T. SELTMAN. Cambridge University Press, 1934. pp. 211. 12s 6d.

The quality of the blocks in this volume of illustrations to the now almost completed Cambridge History, is very much above the standard hitherto followed. Indeed, the illustrations could not be better.

The all too brief Celtic section might have been improved by the addition of Gaulish examples of the red and white painted pottery which is here only illustrated by the Bohemian fragments. This omission is the more important since there were no such instances given in vol. III.

The Parthian period is one of the obscurest of all periods in the history of art and the section of illustrations is the most useful and illuminating in any existing publication.



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The Nimrud Dag reliefs show clearly one of the contributions that went to make Sasanian sculpture.

The Etrusco-Italic section is profoundly controversial and many of the textual descriptions fail to carry conviction. But the material given is adequate and admirable. The bronze head of a boy (a) on p. 43 and the 'Brutus' on p. 47 have no qualities which are not ultimately due to Hellenistic origin. The faces, as racial types, may well be Italic, but I can see no quality other than Greek in the art. Perhaps I am obtuse. Nor do I feel convinced that one can claim the use of death-masks as the basis of stone-carving (admirably illustrated on p. 50) as an Italic peculiarity. As far back as the fourth century Lysistratus had employed life-masks, from which wax moulds were made and worked up. Italians derived the practice as much from Greek technique as from ancestral Italic custom: one cannot differentiate between life-masks and death-masks from an artistic point of view.

The reliefs illustrated on p. 61 are there described as instances of the style and technique of Italo-Etruscan origin. Movement of figures from back to front; penetration of the background by elements of the scene, and solidity of treatment are stated to be the qualities by which Italo-Etruscan work is differentiated from Greek, according to the principles laid down by Sieveking. The 'tri-dimensional Italic principle' is contrasted with the 'two-dimensional Greek'. Frankly I must confess that such an analysis strikes me as nonsense. Movement from back to front is found as early as the Bassae reliefs; penetration of the background is found in the Siphnian reliefs of the sixth century and examples multiply as Greek sculpture moves on its course. The Girgenti sarcophagus, an early Hellenistic work, shows all these alleged Italic qualities combined and fully developed. Roman sculpture merely varies the method and exaggerates it. The claims of autochthonous Italic methods seem to evaporate if Greek sculpture is examined with an eye unclouded by rule-of-thumb principles alleged to be Greek.

The Roman section on sculpture and architecture nevertheless serves as a magnificent corpus for study, and the section on mosaic and painting is of the greatest value.

S. CASSON.

### KANSU MORTUARY URNS OF THE PAN SHAN AND MA CH'ANG GROUPS.

*Palaeontologia Sinica. Series D, III, fasc. 1. By NILS PALMGREN. Peiping, 1934. pp. 204 and 41 plates. Price not stated.*

It was in 1923 that the existence in China of an apparently neolithic culture characterized by painted pottery was first announced. Connexions with the early painted wares of the West were at once suspected and eagerly sought. But in those days neolithic or copper-age painted pottery was hardly recognized save in Thessaly and the Ukraine, at Anau and at Susa; too much significance was liable to be attached to the abstract fact of vases being painted. In the intervening decade other regions have produced painted fabrics no less ancient, a bewildering variety of distinct and apparently unrelated styles has been recognized and a long persistence of painted decoration, notably in Iran, has been established. On the critical principles formulated and applied by Frankfort many of the comparisons which in 1923 seemed surprising now appear childish and irrelevant. Only the analogies between the Chinese wares and those of the Black-earth region of southeastern Europe, first noticed by Hubert Schmidt and more rigorously worked out by Menghin, still hold the field despite Frankfort's criticisms.



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In the same time the position of the painted ware in Chinese prehistory has been to some extent clarified. Andersson distinguishes six neolithic or chalcolithic stages in western China. His oldest, or Ch'i chia stage, lacked painted ware. This appears first in the next stage, termed Yang shao. Other stages, considered to be later on the strength of an increasing wealth in metal, carry on the Yang shao traditions with new forms, designs and techniques. It is well, however, to remember that this cultural sequence lacks stratigraphical support; the prehistoric inhabitants of western China did not occupy the same site for many generations, but repeatedly shifted their villages in obedience to the exigencies of migratory garden culture. Mere absence of metal is not in itself proof of high antiquity, and terms like 'neolithic' must not be invested with the absolute value of geological periods. Andersson himself has noted on one vessel of his earliest, or Ch'i chia stage, painting recalling that of his latest or Sha ching group!

The present volume studiously avoids such controversial questions, but it presents for the first time material in the light of which the presumed western connexions can profitably be discussed. Dr Palmgren has contented himself with illustrating and describing some seventy vessels of the Yang shao style and a like number of the rather later Ma ch'ang group. The majority of the vessels come from cemeteries that had been plundered by local peasants so that closed tomb-groups cannot be expected. Such typological developments as the author traces, therefore, depend on an analysis of forms or motives in isolation and are unsupported by parallel series. Prolonged and intimate contact with this large collection has revealed to Palmgren many secrets of the prehistoric potter's craft. His acute observations show that the pots were built up by rings and that they were fired in kilns in which the vessels might be stacked three deep. Inferences as to the relative ages of the several features may be accepted with more reserve. The Yang shao pots are grouped into 40 forms, their ornamentation into 20 'décor families'. Twenty shapes and 16 'décor families' suffice for the description of the Ma ch'ang group. In the older series the large urns with long or short necks account for 70 per cent. of the vessels described, and spiral patterns or their derivatives predominate in the ornamentation.

The description is completed by 8 superb coloured plates, 33 plates of half-tones and 228 line blocks. From these the reader will gain a fair idea of the magnificent ceramic art of China in a Stone Age; it is ruled by a self-conscious style, not less firm and mature than those of the Indus valley, Minoan Crete or Iran and Syria (where the latter regions achieved a style at all). Its products are genuine things of beauty; the late Roger Fry remarked to the reviewer that the Kansu pottery exhibited the most truly artistic system of ornament achieved by any prehistoric potter.

It is in the light of this style, taking form, composition and motives at once into account, that the supposed relations of Chinese culture to western ones must be judged. The contrast to the art of the Indus valley, Susa I and Arpachiya (Tell Halaf) is evident. With the Ukraine the case is different. The general effect of the big Yang shao urns is really reminiscent of the later Tripolye vases as represented in Cucuteni II or Schipenitz. The Chinese style is indeed more sophisticated than the European; it is not easy to define to what precise phase in the development of the Black-earth designs it should be compared—neither the chaotically arranged spirals of Cucuteni I nor the (? degenerate) by-products represented at Koszyłowce come into the picture. But Menghin's bold hypothesis remains attractive: here in western China we would find descendants of the once flourishing culture that vanished so mysteriously from the Black-earth belt of Europe! Still how should cultivators in whose farm-yards pigs were evidently important



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move with their ceramic art from Europe to Chinese Turkestan without a single intermediate station east of the Dnieper? Migratory garden-culture will not explain a discontinuous spread of this magnitude.

V. G. CHILDE.

SHIRAGI KO GA NO KEN-KYŪ : Study on the Ancient Tiles of the Silla Dynasty, Korea, with a Corpus of these Tiles. By K. HAMADA and S. UMEHARA. Report upon Archaeological Research in the Dept. of Literature XIII. *Kyoto Imperial University*, 1934. Japanese Text about 80 pp. with 43 illustrations, English Summary 20 pp. and 78 plates.

A school of architecture may select a special feature for extra decoration. Chinese architects and their pupils in Korea and Japan selected the edges of tiles overhanging the eaves as a surface for ornament. Chinese tiling is essentially like Greek or Roman made up of the flattish *tegula* and the *imbrex* of semicircular section inverted over the joint of the *tegulae*. At the eaves the lowest *imbrex* ends in a sort of antefix, but it is a complete disk or oval overlapping the edge of the roof, and the lowest *tegula* shows its edge as a crescent or rather segment of a circle. The disks are common enough in museums but have generally lost the semi-cylindrical tile behind them. To complete the roof there must be the great ridge-tiles and special tiles at the corners of the eaves. All these afforded scope for ornament. T'ang art was imitated in the kingdom of Silla (Chinese Hsin-la; Jap. Shiragi) in southeast Korea with its capital of Kyōng-chiu (Ch'ing-chou, Jap. Kei-shū) during its flourishing period from about A.D. 660 to 779. The Silla craftsmen covered all the surfaces along the eaves that could be seen from below with delicate ornament, the disks with lotus rosettes, the crescents and edgings with running sprays going back to West Asiatic and even Greek models. They introduced animals, birds and monsters into the foliage and put monstrous heads upon the disks, upon which the Chinese were fond of putting mere ideograms. The examples of Silla work are classified upon an elaborate system, but the authors point out that the craftsmen were impractical inasmuch as their patterns were too much detailed for their high setting, while the Chinese and Japanese work, being bolder, shows more effectively from the ground. Paving-tiles and building-bricks with similar patterns are also reproduced. The book is dedicated to the memory of Dr A. H. Sayce, whose portrait adorns it.

It may be mentioned that Messrs. Nishida and Umehara published last year in the 'Report of Investigations into the History of Kyoto City', vol. xv, an account of a kiln for producing such tiles discovered on the site of the ancient palace Kurusu-no-kama at Kyoto belonging to the Heian period early in the 9th century.

ELLIS H. MINNS.

ANCIENT TEXTILES FROM EGYPT IN THE UNIVERSITY OF MICHIGAN COLLECTION. By LILIAN M. WILSON. *Ann Arbor: University of Michigan Press*, 1933. With 23 plates and 2 text figures.

This book describes two collections of textiles, one from the excavations directed by Mr Starkey at Karanis (Kom Aushim) in 1924-26, and the other purchased from a dealer.

The first collection is the more important because the fragments in it, which were found in the rubbish of small houses, can be dated, from the coins and papyri found with them, from the end of the 3rd century to A.D. 460. At the close of the expedition they were sent to Mr Thomas Midgley, who cleaned and classified them.



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A selection from both collections is here figured in excellent collotype plates, and full descriptions are given by the author, Miss Wilson, as well as chapters discussing the different weaves and the loom which may have been used to produce them. Among the mass of fragments from Karanis (3450 in all), mainly in plain and tapestry weave, a few pieces are classed as 'twills' and 'draw loom' weaves, and Miss Wilson claims that these 'establish a *terminus ante quem* for the introduction of the horizontal loom'.

The twills alone could not prove this, for they can be woven on more primitive looms, e.g. the twills of the Navahos and other Indians. The weaves nos. 16, 17 (pl. 3), however, are in a class of all over repeating pattern that well supports Miss Wilson's contention, but it seems unnecessary to call them 'draw loom' weaves, as they appear, as do also some pieces kindly shown me by Mr Midgley, to be capable of production on heddles alone, without a second shedding device.\* While the fragments are too small to prove this, they do make it probable that the horizontal treadle loom dates back to the 4th century A.D.

There are some defects in the book: the chapter on Ancient Looms shows misconceptions of which the discussion cannot be embarked on here, and there are no diagrams to assist descriptions of the special weaves. But the value of the work is evident.

Students of primitive weaving are much indebted to those who have contributed to the volume, to Mr Starkey for preserving the material, and to the University of Michigan for breaking new ground in its publication.

G. M. CROWFOOT.

**CARAVANS OF THE OLD SAHARA:** An Introduction to the History of the Western Sudan. By E. W. BOVILL. Published for the International Institute of African Languages and Cultures. Oxford University Press, 1933. pp. 300 and 13 maps. 21s.

Mr Bovill's work is extremely difficult to review and would take considerably more space than is available for the comment and criticism which it really deserves. It is the first attempt yet made to present in a lucid and coherent form a historical picture of Equatoria from the Atlantic to the Nile Valley during two millennia of movement and change. Mr Bovill has succeeded in writing the first standard work on this great subject. It suffers inevitably from the defect of its virtues, since within the compass of one volume there is no room for the discussion of data and evidence where these are conflicting and obscure. The subject, moreover, is so little known to the general public that the Author is unable to assume knowledge which in better known areas and historical periods are the common property of any educated reader, and consequently he is obliged to start with first principles. As such, the earlier chapters deal with Roman Africa and the 'Races of Man'. They are inadequate, while the detail available for the last part on the opening up of Equatoria is disproportionate to the amount of evidence available for the main portion of the book, which deals with the enthralling history of the Niger empires.

These defects inherent in the plan are probably unavoidable in any attempt at a survey which commences in prehistoric times and ends with the beginning of this century. It would perhaps have been preferable if the volume had been confined to

\* Worst, *Foot Power Loom Weaving*. See 'Monk's Belt', a 4 heddle weave, p. 46, and damasks on 10-12 heddles, pp. 153, 155.



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the history of the great African empires which extended in succession of time and place from the Upper Niger to the Upper Nile with all the influences to which they subjected, and to which they were subjected by, the desert people of the Sahara. It is to be hoped that the first and the last sections may one day be incorporated into separate volumes.

To anyone who has followed the history of the people and areas concerned there are a thousand matters of detail and not a few of import where the reviewer, and other students of the subject, will differ from Mr Bovill's interpretation of authorities, but in no case will the reader be left with the impression that he has ignored available data or strained uncomfortable evidence. On the contrary, the reader will remain profoundly impressed with the erudition and honest scholarship that this work displays.

The attractive story of the Moorish invasion and conquest of the Middle Niger—one of the most surprising feats in history inasmuch as an army was marched across the western Sahara—is dealt with too briefly to do more than whet one's appetite. Again, the political and territorial organization of the Songhai government of Gao on the Niger—an example of a most enlightened solution of the difficulty of administering different racial unities occupying the same area—is a subject which, properly expanded, might serve as a very interesting object lesson to modern statesmen. There is, perhaps, slightly too much space, comparatively to the other subjects, devoted to the disputes of the autonomous rulers of Timbuctoo. The influence of the desert confederations (or the lack of it from time to time) on administrative policy and dynastic succession in the kingdoms of Negroland is more worthy of study in a concise survey of this kind than anecdotal accounts of isolated early European contacts.

There are other problems to which only further research can provide a solution. Of these the greatest and most fascinating are those presented by the Gara and the Fulani peoples. Ethnologically and anthropometrically, little, it would seem, can be done to elucidate them: the racial cocktail has been too well shaken. There is hope, nevertheless, that diligent collection of oral history and legend and the study of sociological and psychological anthropology in the Niger territories may reveal further data from which some picture of their racial origins and history can be drawn before they are all forgotten. While the gold rush of the last year or two may help in locating the mother reef from which the alluvial deposits of Wangara draw their gold dust, yet another fascinating problem is that of the whereabouts of the copper mines of Tekadda somewhere in the steppe desert between Agades and Sokoto.

These are only some of the exciting and controversial subjects touched by the Author. The great merit of his work is that he has provided a frame into which the missing pieces of the jigsaw puzzle may be fitted as they come to light and their proper places are recognized.

The detail-maps which appear frequently in the text are admirable, but are by no means adequate substitutes for a really good map of the whole territory covered which should be provided at the end of the volume and so mounted that it may be consulted while reading the text. But this is the publisher's fault doubtless and not Mr Bovill's, who also cannot have had any hand in choosing the deplorable and rather misleading title, which is calculated to prejudice so scholarly a book. The reviewer's task has been particularly invidious and hard. He feels he cannot do justice to such a book and, furthermore, suffers from the coals of fire attendant upon seeing his own researches quoted in extenso and in some matters with misleading consequences, having come to realize as he has that Mr Bovill has accepted in good faith certain conclusions which unfortunately the reviewer now suspects are incorrect.

FRANCIS RODD.



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ARCHAEOLOGIA ORIENTALIS, published by the Far Eastern Archaeological Society in Tokyo and Kyoto.

FOLIO SERIES (see *ANTIQUITY*, March 1935, p. 121).

IV. YING-CH'ENG-TZU: the Han Brick Tomb with Fresco-painting, etc., near Chien-mu-ch'eng-i, S. Manchuria. By OSAMU MORI and HIROSHI NAITO with an English Résumé by KOSAKU HAMADA. 8 pp. in English, 36 §§ in Japanese and two Appendices, 45 plates (11 coloured), 38 figures. 1934.

This is a description of two tombs on the road between Port Arthur and Dairen. No. 1 was discovered by chance in making the road: it consisted of three vaulted chambers in brick; an arch between the main room and one of the side rooms is horse-shoe shaped, which is a novelty. It contained practically nothing but pottery, everything else had been plundered. The date was obviously Han.

The other tomb, no. 2, was more interesting. Its plan is original, a central chamber and three side-chambers; within the central chamber an inner chamber or tabernacle of brick contained the main burial; an inner chamber of wood occurs in Siberia. A model of the whole has been set up in the museum at Port Arthur. The inner faces of the main walls were in patterned brick coloured red, white and dark brown: this work is the best of its kind yet found and is well reproduced in the frontispiece. But the great feature of the tomb is wall-painting, described as fresco, in black and red pigment. The principal scene seems to show people greeting the dead man and offering him food and drink. The other subjects are life-size warriors and beasts guarding the doors of the chamber inside and out. They are very rude but rather spirited and practically no Han painting on such a scale is known: in character it is just the opposite to the contemporary work on the lacquer basket described in *ANTIQUITY*, June 1935 (p. 251). Among the grave-goods, again mostly pottery, the best were a five-socketed candlestick and a model house. The publication is as perfect as the former issues of the Society. The English summary is rather jejune.

SERIES B (small quarto).

I. INNER MONGOLIA AND THE REGION OF THE GREAT WALL. By NAMIO EGAMI and SEIICHI MIZUNO, 16 pp. in English, Japanese text in three parts: I. Microlithic culture in Mongolia, 62 §§, 15 plates and 27 figures; II. Sui-yüan Bronzes, 204 §§, 48 pl., 116 figures; III. Cord-ware sites along the North Frontier of China, 40 §§, 66 pl., 31 figures and map. 1935.

I. The microlithic culture of the sand country in Mongolia and Manchuria is known from the work of Licert, Granger and Nelson, and Titov and Tolmachëv. It had not been observed on the R. Silingol (116° W, 44° N). It appears to be the culture of quasi-nomads contemporary with the Chinese neolithic husbandmen and lasted through the 2nd millennium B.C. to be superseded by the Ordos bronze culture. Associated with the microliths are perforated stones, probably hammers. An interesting map (fig. 27) shows the regions of neolithic and microlithic and their overlap along a belt stretching from Kansu to Vladivostok.

II. The Sui-yüan Bronzes (for which Professor Andersson has decided to use the name Ordos) have received much attention lately. The best collection ever made was shown at the Stockholm congress two years ago. Andersson published an immense number in the *Bull. of Far Eastern Antig.* vol. IV and Dr Arne two well-observed finds in vol. V. Then Dr Salmons astonished us all by his book, *Sino-Siberian Art*, on the



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Loo collection, in which he wished to bring the dates down from say 300 B.C. to A.D. 1100, but did not convince us.

Our Japanese authors begin with a well-illustrated account of the whole subject; most of the text-figures are drawn from European publications. They are inclined to see the origin of both style and types in the far west—in the table, § 200, they make the culture begin about 800 B.C., flourish 500 to 100 B.C. and survive more or less till A.D. 200. Their judgment is mainly based on Teploukhov's survey of styles in Minusinsk. They agree with the common view which assigns the things to the Hiung-nu or Huns.

The plates mainly illustrate the collection made by the authors and now divided between Tokyo and Kyoto, though a few objects in other museums or in private possession are reproduced. It contains little but axes, daggers, knives and cauldrons and is rather poor in the more decorated pieces such as the belt-plaques which show the style at its most elaborate. A type that I have not noticed before is that of mace-heads with angular projections. The objects were unfortunately not excavated by the authors, but mostly bought at Sui-yüan or Peiping. What we most need is precise information as to how these things are found.

Three plates are devoted to more or less schematic drawings of (i) the socketed axes and chisels, (ii) daggers and (iii) knives, classified under types, and a useful attempt is made to indicate which types come from the steppe region and which from the basin of the Huang-ho. This section of the book is helpful as making known some new material, but does not seem to add very much to our knowledge, though no doubt very valuable for Japanese readers.

III. A description of dwelling sites with corded ware and polished celts all along the railway from Peiping by Sui-yüan to P'ao-t'ou. The people were the humble Chinese, for the most part contemporary with the bronze age, but perhaps not using metal themselves and so sub-neolithic, lasting down into Han times, apparently living among the nomads and encroaching upon their pastures, just as they were doing the other day. This stage was followed by one in which the nomads after Han times got the better of the Chinese. So the alternation goes on.

This Series B promises to be very useful.

ELLIS H. MINNS.

A CONTRIBUTION TO THE ARCHAEOLOGY OF NORTHEAST GREENLAND. By SØREN RICHTER (*Skrifter om Skibard og Ishavs-undersøkelser*, no. 63). Oslo (*Det kong. Departement for Handel, Sjøfart, Industri, Håndverk og Fiskeri*), 1934. pp. 149. 25 kr.

It is refreshing to find a government Department for Trade, Marine, Industry and Fisheries publishing an archaeological monograph. That is what the Norwegian Department in question has done. And very courteously the valuable monograph is written not in Norwegian but in English. In 1929-30 and 1932-3 Mr Richter accompanied hunting expeditions to northeast Greenland especially to study archaeological remains. During these journeys he was given the opportunity of excavating deserted Eskimo dwelling places. In this official publication he gives an admirable account of his results. Two sorts of structure were encountered at the old settlement sites. The winter-houses are excavated in the earth but walled with stones and roofed with drift-wood. In plan they recall the familiar snow-houses of the Arctic Eskimo, but also dwellings erected both for the living and for the dead in Scotland as well as the chamber



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tombs of other parts of Atlantic Europe. But such subterranean dwellings were habitable only in winter: every spring the thaw would convert them into 'marshy burrows'. Then their builders removed into tents, pitched in the vicinity: their sites are marked today by rings of stones reminiscent of our hut-circles (in Scotland the juxtaposition of *souterrains* and hut-circles has often been noted).

The winter-houses yielded many hunting implements of bone, knives and vessels of bone, stone and wood and curious ornaments. These are well illustrated and are interesting for their similarity to prehistoric European artifacts: the 'snow knives' of fig. 56 for instance are curiously like 'neolithic' knives from Scandinavia. Besides describing and illustrating the ancient dwellings and their contents the author lucidly discusses their geographical situation, and demonstrates how 'the winter homes of the Eskimo were not chosen casually but with a detailed knowledge of the country and of the facilities the site offered for getting through the dark months in the best possible manner'.  
V. G. CHILDE.

NOTES ON THE HISTORY OF ANCIENT ROADS AND THEIR CONSTRUCTION. By R. J. FORBES. Amsterdam: Allard Pierson Stichting, 1934. pp. 182 and 35 text-figures. Price not stated.

We welcome this scholarly and fully documented essay on the subject of ancient roads and road-building, which is published by the Archaeological Department of the University of Amsterdam with financial support from Sir Henri Deterding of the Royal Dutch Petroleum Company.

There is no aspect of road-building in ancient times which is not discussed in detail by the author—Malta and Crete, Egypt and Palestine, Asia, India, Greece, and Italy are all considered in his wide survey. There are further chapters on the prehistoric roads of Europe, and the geographical considerations of road-building, various chronological tables, and finally a most extensive bibliography of ancient and modern sources.

It is absolutely essential that any road-book should be furnished with clear, well drawn maps, and here Mr Forbes has departed from the high standard of his text in publishing such altogether inadequate maps as fig. 3, 'The Trackways of Engeland', fig. 2, 'Traderoutes in Prehistoric Europe' and fig. 21, 'Roads of Roman Britain'. Not a single map in the book possesses a scale, and only one is provided with a north mark.

Especially to be commended is the chapter dealing with the prehistoric log-roads of Europe. Mr Forbes quite rightly reminds us that the wide variations in constructional detail should be clear enough evidence that they cannot all date from one and the same period. In Holland, for instance, many of them occur under the 'boundary layer' of the peat moor and are thus early Bronze Age in date, while further east in Prussia log-roads were still being used in Hallstatt—La Tène times, and in the wooded and swampy country beyond the Rhine they were frequently used by the Romans in their constant activities for the defence of the frontier against the German tribes. Altogether, there can be no doubt that simple corduroy roads were among the earliest experiments in road-making, and that their outstanding advantages of simplicity, cheapness, and ease of construction have been appreciated over a long period, and even down to the present day.

It is curious at first sight that the marked technical ability of the Greeks made no contribution to road-building. The answer to the paradox, as Mr Forbes shows, is that



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the social and geographical structure of Greece actually hindered road-building, for the political horizon never reached beyond the city and its valley. At any rate, Greek travellers were accustomed to some sort of comfort in the inns along the roads, for we are told that 'roads without inns are no better than life without holidays'.

There are several small matters which will need revision in a future edition. The Pilgrim's Way was not immortalized by Chaucer (p. 27), *Radial Times* (p. 168) should surely read *Radio Times*. One misses Collingwood's *Roman Britain* from the bibliography of Roman roads; and there are various trifling matters of spelling, grammar, and idiom which are tiresome to an English reader. For all this, it is a book which no future writer can afford to ignore.

R. F. JESSUP.

**SYMBOLS FOR DESIGNERS:** a handbook on the application of Symbols and Symbolism to Design. By ARNOLD WHITTICK. *Lockwood*, 1935. pp. xvi, 168, 15 plates and text-figures. 12s 6d.

Mr Whittick is not a little concerned to distinguish between conscious and unconscious symbolism in order to explain to his reader that he intends to discuss that variety of symbolism which is used consciously and deliberately. Symbolism, unconscious in its beginnings, must satisfy an instinctive susceptibility; it is, in the author's own words 'an expression of man's adaptation to his environment', and this is the underlying proposition of Mr Whittick's clever introductory thesis which occupies the first part of the book.

The main part of the book is an attractive and well carried out dictionary of symbols, their history, meaning, and suitability for use in modern design. It is, of course, for designers and craftsmen that the book is chiefly intended, though the curiously-minded general reader will find a good deal to interest him. Many excellent illustrations are provided, but it is a pity they could not have been extended to cover every symbol which is considered.

This book appeared during a recent exhibition at Burlington House; after one had contemplated that somewhat distressing and fulsome liaison between art and industry, it was refreshing to open Mr Whittick's book and to see that commercial artists can still create a refined and beautiful symbolism in such works as the Mond Anniversary Medal and the Spaeth memorial at St. Louis.

R. F. JESSUP.

**EUROPEAN CIVILIZATION:** its Origin and Development. By Various contributors. Under the direction of Edward Eyre. Vol. 1: Prehistoric Man and Earliest Known Societies. *Oxford University Press*, 1934. pp. x, 1322. 25s.

'The real injury to an appreciation of what the past has been comes from . . . the assumption of the inevitability of what in fact took place, and the habit of judging past events according to the way they seem to have hastened the coming of the modern world of sovereign, territorial, democratic states. The unity of Europe has been less vividly apprehended than the divergences of its parts'. The imposing series of volumes which we are now promised is intended to restore the balance. Their purpose is to tell the story of unity rather than of separation; to insist upon the living, common elements in European civilization, and to show how they originated and grew. The enterprise is formidable, as formidable as the task of criticism when each of the huge volumes appears.

How is this first instalment of 1322 pages conceived? It is, I think, a pity that the editor did not attempt to give, in his brief introduction, a coherent summary of the



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scheme that was in his mind. What he has in fact done is to get his contributors to describe the rise of two parallel traditions. On the one hand they have traced the emergence, from primitive civilization, of a culture that was to spread the notion of the universe 'as reasonable and as manageable by human reason', alterable by human interference; to show that the circumstances and processes of nature 'are not only in themselves orderly, but that this orderliness is intelligible to man'. This we might accordingly term the tradition of the reasonable universe. Along with it, but apart from it, developing in and through the peculiar history of a people that became a fiercely self-conscious nation, is the tradition of holiness, the religious tradition proper, in the sense that religion does not imply merely the service and worship of the deity, but undefilement and moral excellence on the part of the worshipper. The first of these traditions culminates, after the rise and fall of preliminary and preparatory older cultures, in the achievements of the Greeks; the second is realized—we cannot speak of fulfilment yet—in the history of Israel. Presently, in the Roman world, there will be interpenetration: in the writer of the Fourth Gospel, in St. Paul and in the early Fathers, the two traditions will be seen in varying measures combined, *ratio and iustitia*, the Greek (in its Hellenistic-Roman setting) with the Hebrew (in its redemptive-Messianic). The union is the work of Christianity in the Mediterranean world.

This is unduly to simplify, perhaps unfairly to anticipate. After all, there is a more material reading of the book: for if it tells the story of how people settled down to civilized life round the great distributing medium of the Mediterranean, that is enough: and this task has certainly been performed. But the reader needs more help than he has been given. When he leaves the chapters in which Professors Myres outlines the growth and distribution of the Indo-European-speaking peoples (chapters which, to one who has long known him, seem to sum up and unify years of research), he may feel a trifle lost in the lengthy section on 'the East' by M. Jean, and wonder what is its relation to the very full analysis of the Old Testament written in the true spirit of orthodoxy by Dr Michael Gruenthaner;\* or how the story of Ancient Egypt, told so clearly by the late Professor Peet, is related to either. A good table of comparative dates would have been very valuable; or maps showing more than areas of speech or culture (features both well represented here), in fact actual reconstructions of invasions and migrations. If however the guiding lines suggested above are a fair and trustworthy interpretation of the book, then I feel that unquestionably the vital sections are these: first, the pages in which Professor Myres, seated aloft, as it were, in a great aeroplane driven by the three engines of archaeology, geography and linguistics, flies over the original homes of the Indo-European groups, and selecting the most interesting and significant section of them, the 'battle-axe folk', shows how they spread from the South Russian Steppe to the Baltic and Central Germany on the one hand, and to the Balkans and Asia Minor on the other, moving out of their 'grassland cradle' mainly owing to the climatic changes that took place after 2500 B.C.; and how too it was largely conditions of climate, the rain-crisis of 1400 B.C., that determined the exodus of Greek-speaking people from the highlands west of the Aegean to the coast-plains. Secondly, pervading M. Jean's narrative of early Assyria, Egypt and Canaan, the emphasis laid upon early religion as a *social and political* force, especially as illustrated in contemporary poems and prophecy. And thirdly, Mr Gomme's account of the spread of Hellenism, which is

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\* We are informed that this chapter has now been withdrawn and the volume reissued without it.—EDITOR.

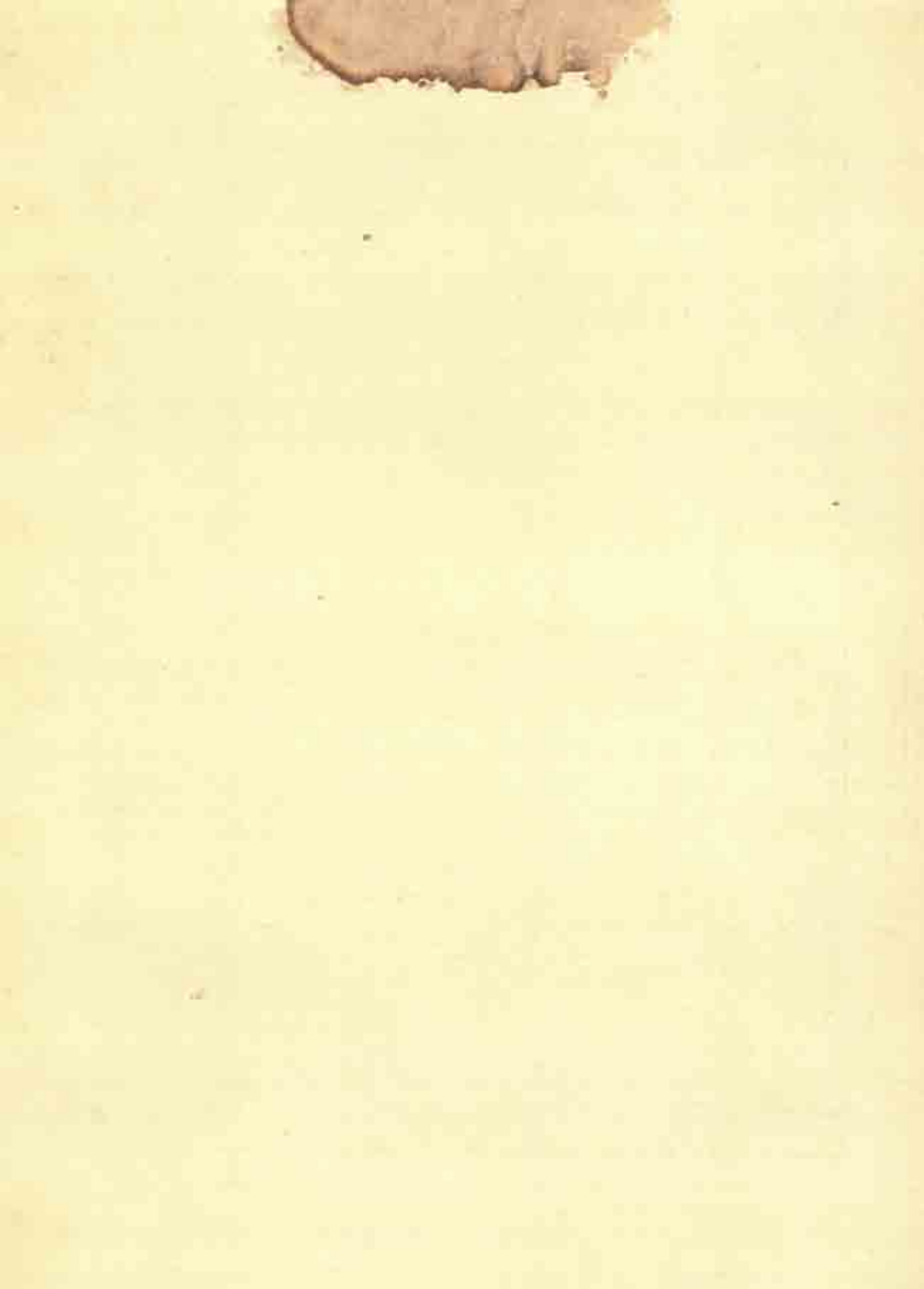
## ANTIQUITY

ampler and better than his too summary chapter on art, letters and philosophy. We could have spared many of the pages on Israel and Judah (for M. Jean has thoroughly explained the significance of the Hebrew prophets) for more discussion of self-conscious humanity, of the notion of personality, the concept of taste, flowering in the Greek world. At the beginning Dr Schmidt vindicated the humanity of primitive man: would it not have been well to devote more space to man fully grown? E. F. JACOB.

## Books Received

- ROMAN MINES IN EUROPE, by Oliver Davies. Clarendon Press, 1935. pp. xii, 294, and 49 text-figures, 6 maps. 30s.
- SENNACHERIB'S AQUEDUCT AT JERWAN, by Thorkild Jacobsen and Seton Lloyd. University of Chicago Oriental Institute Publications, vol. xxiv. University of Chicago Press, 1935. pp. xii, 52, 12 text-figures, 37 plates. 22s 6d.
- EARLY WELSH GNOMIC POEMS, by Kenneth Jackson. Cardiff: University of Wales Press Board, 1935. pp. x, 70. 3s 6d.
- LLAWLYFR HEN WYDDELEG, gan G. MELVILLE RICHARDS. Cardiff: University of Wales Press Board. pp. viii, 136. 6s.
- IN THE SEALED CAVE, by Louis Heintman. Williams and Norgate, 1935. pp. 227, 5 plates. 6s.
- HOMMAGE A HORACE A L'OCCASION DU BIMILLÉNAIRE DE SA NAISSANCE. Les Études Classiques, vol. 4, January 1935. Namur: rue de Bruxelles, 59. pp. 208.
- THE UNIVERSITY LIBRARY, UNIVERSITY OF LONDON. Catalogue of books on Archaeology and Art and cognate works belonging to the Preedy Memorial Library and other Collections in the University Library. Section 1: Archaeology and Ancient Art. London: published by the University of London, 1935. pp. xvi, 227.
- RELIGION IN VIRGIL, by Cyril Bailey. Clarendon Press, 1935. pp. viii, 338. 15s.
- THE DORSET LANDSCAPE, by Geoffrey Clark and W. Harding Thompson. A. and C. Black, 1935. pp. xii, 116, 16 plates, 4 maps and diagrams. 5s.
- DREAMS IN OLD NORSE LITERATURE AND THEIR AFFINITIES IN FOLKLORE, by G. D. Kelchner. Cambridge University Press, 1935. pp. x, 154. 10s 6d.
- CIVILISATION AND THE GROWTH OF LAW, by William A. Robson. Macmillan, 1935. pp. xvi, 354. 12s 6d.
- L'APOCALYPSE ET LES CULTES DE DOMITIEN ET DE CYBELE, par P. Touilleux. Paris: Geuthner, 1935. pp. 192. 30 francs.







CORBEL-HEAD FOUND ON THE SITE OF BURY ST. EDMUND'S ABBEY (See p. 388)



# Antiquity

## A Quarterly Review of Archaeology

VOL. IX No. 36

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## Editorial Notes

WE return once again to museums, in the two-fold belief that the subject will repay repeated attention, and that our readers for the most part share our view about its importance. We believe that the problem of the museums of this country must sooner or later receive organized attention, and we make no excuse therefore for thus returning to it. Moreover, museums in their present form must be a cause of increasing anxiety to those who are interested in the preservation of the raw materials of archaeology.

The accumulation of material has been hastened during recent years by the vast amount of scientific excavation which has been carried out. Added to the older collections, which consist very largely of chance finds made by individuals, it is leading rapidly to the congestion of our older and larger museums. And the problem is not simply one of bulk. The exhibition of material obtained in the age of scientific exploration, which we hope has now begun, calls for systems which were not formerly necessary. Plans, diagrams and photographs are all essential if a complicated site is to be properly understood: its stratigraphy and other features are at least as important as the specimens themselves. Without adequate means of museum display results will be wasted in one sense; for adequate exhibition is scarcely less valuable than detailed excavation reports. The latter are usually only

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read—if they are read at all, and not merely used as reference-books—by specialists; but that much maligned person the ordinary man (upon whom, after all, these things ultimately depend) must be provided for by museum exhibits designed to encourage his interest in the doings of archaeology. Not, of course, that such exhibits will interest only the 'ordinary man': properly assembled they are of untold value to the expert archaeologist also.



In their present congested state our larger museums, with the British Museum at their head, are beginning to be unable to cope with the demands made on their space by these new methods. Their collections are rapidly outgrowing—indeed, judged by modern methods of display, they have already outgrown—buildings which in most cases were designed on rigid 'classical' lines to conform with an outworn tradition, with no regard for subsequent expansion. Material has been poured into them until bursting-point has almost been reached. Their custodians struggle, in the face of congestion and unsatisfactory appliances, to display their collections according to methods which they recognize to be more in keeping with modern ideas and aims. But in present conditions such efforts, praiseworthy though they are in themselves, are doomed to ultimate disappointment. The disease is too deep-seated for temporary measures; what is needed is nothing short of a major operation.



Two methods of tackling the problem present themselves: new buildings, and some kind of reorganization and redistribution of collections. The new buildings are bound to come. When they do, we hope that they will be designed expressly to fulfil their purpose as museums, and nothing else. To say this may seem unnecessary, but we are not sure that such a museum has yet been built anywhere in Europe upon a large scale. Architects still devote their ingenuity to expensive and wasteful effects, concentrating upon the setting to the detriment of what it is to contain. (Incidentally, we suspect that museum-curators themselves are not altogether without blame in this respect, for failing to insist upon due weight being given to their own requirements). Recent experience, however, has already made several points clear. In the new buildings the separate needs of education



## EDITORIAL NOTES

and research must be clearly distinguished, by providing galleries for display and 'reserves' for students. (It should not be necessary for the ordinary visitor to wade through masses of material which are quite irrelevant to his needs—in any case he will not do it). There should be facilities also for the examination of maps and photographs, cinematographs, as well as other amenities calculated to encourage the ordinary visitor and to sweeten the labours of the professional expert.



We hope in due course to publish an article on this question of museum buildings, but we do not believe that new buildings will entirely overcome the difficulties to which we have referred, at any rate in London. With them should be combined some scheme for splitting up the collections into a number of museums, suitably placed in different parts of London—this might be done in conjunction with slum-clearance schemes—which would bring into circulation the quantity of stagnant duplicate material which at present either serves to overwhelm the visitor to the galleries or is stored more or less inaccessibly in basements. Carried out with care such an arrangement need in no way incommode the research worker, while it would also dispel the fears of the war-minded archaeologist who sees the record of our past wiped out by a single bomb because all our eggs are in one basket.



The same principle could be extended in favour of the smaller museums elsewhere, although we do not propose at the moment to go into the various problems connected with these. We have not been able yet to see why specimens which serve no useful function in the central museums could not be transferred more or less permanently to the appropriate local museums, as long as they are efficiently maintained. This, of course, is quite a different thing from the loan of ordinary circulating material. It would presumably involve the repeal of the law relating to the British Museum collections, under which it is forbidden to dispose of even worthless material when once it has been registered. Such a step in itself would have a salutary effect, because it would establish the principle that these problems should be the care of trained experts, while it would also have an immediate bearing upon the problem of congestion.

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Our frontispiece is a carved corbel-head found during recent excavations on the site of the Abbey of St. Edmund at Bury St. Edmund's, Essex. (We regret to learn that 'these have been conducted in a haphazard manner, with no form of qualified supervision'). It was found beside a passage with a tiled floor. The head is 8 inches from the chin to the top of the cap, is in perfect condition, and bears a trace of colour in the nostrils. Opinions differ with regard to the date. On the grounds that there are two very similar heads on the main porch of St. Mary's Church, built in the early 15th century and within the Abbey precincts, a 15th century date has been suggested. On the other hand some very high authorities prefer a date about 1300. Whatever the precise date, it is a fine example of medieval work, and as such we present it to our readers as a kind of Christmas card, to relieve the monotony of undiluted archaeology.



The completion of another volume of ANTIQUITY brings us once more to the time when we ask our subscribers for the renewal of their much appreciated support, and direct their attention to the notice printed below. We would also say that an early response is a very considerable help to us.



### VOLUME X, FOR 1936

*A renewal form for subscriptions for 1936 is inserted in this number and we shall be very glad if our subscribers will return it with their cheques as promptly as they may find convenient. The forms are omitted from copies sent to subscribers who pay through banks or who have paid for 1936 in advance.*



## The Newton Stone

by R. A. S. MACALISTER

*Professor of Celtic Archaeology, University College, Dublin*

IN a paddock in the garden of Newton House, a few miles from Aberdeen, there stands a pillar-stone, bearing on one of its edges, and on an incised supplementary line, an inscription in a long and a short row of Ogham characters; and on an adjacent face a second inscription, in six lines of apparently alphabetic characters (PLATE I).

Decipherer after decipherer has visited it; published an interpretation of it; and then they, like the Baker, 'softly and suddenly vanish away', as though scared by their own temerity, leaving the arena vacant for the next-comer. No one has troubled to dispute any of these readings: no one, not even any of the authors themselves, has ventured to defend them. There has never been a 'Newton Stone' controversy; the literature of the subject, like that of the 'Number of the Beast', resembles a series of disconnected runaway knocks, inflicted by street urchins on the door of a tempting corner house. The redoubtable General Vallancey began the game, by discovering that the inscription commemorated 'Gylf Gummara'. After him, we have been permitted momentary glimpses of a Phoenician magistrate, with the improbable name Han-Thanit-Zenaniah, who, for some reason, was 'saturated with sorrow'; of another Phoenician, Atalthan son of Pazach, commemorated by his lamenting mother; of an imposing personage who called himself 'the all-powerful O Aremin', but who unexpectedly thought it worth his while to record that he had put a 'gomodrach' on the field—'gomodrach', we are told, being 'Celtic' for nothing more exciting than 'a flock of sheep'! Next comes one Aittie, apparently a Buddhist missionary, who was 'the light of the darkness of a perverted people', until he was unfortunately 'smitten when Baal ruled Jutland'; and following him comes a mysterious potentate described as 'the princely ruler of the Border of the Cumbrians', whose 'home-living niece-daughter' (whatever that means) erected the stone to his memory, with its inscription in a language obscurely specified as 'Yore-Welsh'. One decipherer reads into the inscription a command to 'draw nigh to the soul of Moluag', though he does not tell us how we are to set about doing so; while

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from others we learn of a polysyllabic Celt called Diglovoceus son of Siloqounnos ; of a monosyllabic Saxon called Grig ; and of a number of other phantasms, no less fantastic, whose peaceful slumbers in Limbo we need not disturb.

In a word, the inscription has been before the public for over a century, and every one of the attempts at deciphering it has been philologically impossible—one of them actually gave ' Unggi ' as the genitive of the Gaelic name Angus !—and psychologically ludicrous.

And yet, so far as legibility is concerned, the inscription is only just short of what numismatists call ' mint state '. Moreover, the number of scripts and languages to which it could reasonably be assigned is small, and with one exception these are all more or less familiar to scholars. That one exception is Pictish ; but if this inscription be a specimen of the unknown Pictish language and writing, why is there not a single example of the same alphabet anywhere else in Pictland, among the host of elaborately sculptured monuments that we find there ? If the Picts had such a highly developed alphabet, why did they cumber themselves with the unadaptable Ogham ? If the script is Pictish, these questions are unanswerable ; the obvious inference must be that, whatever it is, it is *not* Pictish.

Of other possibilities there appear to be four, and (in descending order of probability) these are :

- (1) Latin, or a Celtic language, in Roman or minuscular script.
- (2) A Teutonic language, in Runic script.
- (3) Greek, in Greek script.
- (4) Some Oriental language, in its proper script.

To all of these the same criticism applies. These languages are known to scholars ; and even if the eccentric forms of the letters had put minor difficulties in the way of interpretation, some competent decipherer should have come forward long before this, with authority to tell us, at least in general terms, what the inscription is about. A Palmyrene inscription was found in South Shields many years ago ; and it was translated immediately, and as accurately and easily as if it had been found in Palmyra. Contrast the two Phœnician translations of the Newton stone ! I know nothing of the competence of their respective authors to deal with a genuine Phœnician inscription ; but in their efforts at interpreting the Newton inscription, they agreed in their transliteration of only 13 of its 45 letters, and their translations had not a single idea in common. Nor had either of them the slightest resemblance to the ordinary formulæ of Phœnician inscriptions.



## THE NEWTON STONE

When we attempt to reconstruct the history of the stone, we have at our disposal the following evidence :—

(1) A letter written by the (fourth) Earl of Aberdeen, dated 10 September 1855, to Stuart, the editor for the Spalding Club of *The Sculptured Stones of Scotland*; and published in that work (vol. 1, p. 2).

(2) A note in *The Gentleman's Magazine*, October 1807, p. 913, signed 'R'; accompanied by a facsimile (for its time of remarkable excellence) of the alphabetic inscription.

(3) A short note in the 'Advertisement' prefixed to Pinkerton's *Inquiry into the History of Scotland* (ed. of 1814, p. XIII).

We gather that the stone stood on an open moorland, and attracted no special attention, till about 1750. About half a mile to the west there was another stone, bearing conspicuous 'Pictish' symbols; this was moved to Newton House about 1770 ('upwards of sixty years ago', writes Stuart in 1836). We might have expected a neighbouring stone with such a remarkable inscription to have been appropriated along with it; but it was still on its original site when Stuart first saw it in 1835, so that it cannot have been moved before 1836 at the earliest. The two stones now stand side by side, in the paddock at Newton House.

About 1750 a plantation of fir-trees was established around the inscribed stone, and these grew and hid it from view. So little importance was attached to it locally, that it seems thereafter to have been temporarily forgotten: Lord Aberdeen says 'I think it was in the year 1804 that I first saw the Newton stone, the inscription on which I believe had been discovered by some shepherd boys in the preceding year. The stone, at that time, was situated in a fir plantation, a few paces distant from the high road'. This high road (from Aberdeen to Huntly) had been constructed very shortly before, making the site of the stone more accessible, and, probably, reducing the extent of the plantation. 'R', in *The Gentleman's Magazine*, tells us that (in 1807) the stone was 'in a field'—not a plantation. Pinkerton, in 1814, speaks of it as being 'in a small thicket', and describes the discovery of the inscription as 'recent'. In weighing this evidence, we must remember that 'R' is contemporary, and Pinkerton nearly so; whereas Lord Aberdeen writes from memory after half a century.

The long line of Ogham writing is ancient: but I have not the slightest doubt that the shorter line, as well as the six lines of alphabetic signs and wonders on the face of the stone, are recent, and that they were executed while it was under cover of the plantation. They

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belong to the time when owners of estates built mock ruins and 'follies'; and I take them to belong to this class. The perpetrator must have been of the type of a certain Edward Hudson, who owned a property at Rathfarnham, near Dublin; where he amused himself by constructing sham dolmens, among which he erected a pillar-stone inscribed, in Oghams, with the following appropriate tag adapted from Horace (Epist. i, xiv, 39):—

*Rident vicini glebas et saxa mouentem  
Eduardum Hudson.*

At the first glance we see that the technique of the two inscriptions is essentially different. The Ogham scores are pocked, the commonest



THE ALPHABETIC INSCRIPTION DIVIDED INTO ITS COMPONENT PARTS

way of fashioning ancient inscriptions in these countries; the alphabetic letters are cut in grooves, with a v-shaped section, by means of a mason's chisel. The stone-cutter was expert at his task: this is important to notice, for we cannot take refuge in the assumption that the inscription is unintelligible because the letters were distorted by an ignorant blunderer. In any case, it is impossible to believe that the man who instructed a mason to cut so elaborate an inscription would have rested satisfied with the result, if neither he, nor those whom he desired to address, could have read it! He would surely have ordered its destruction, and commissioned some more competent artificer to carry out the job.

A fact of primary importance has been overlooked by decipherers<sup>1</sup>: namely that the inscription in alphabetic characters is really two

<sup>1</sup> Rhys hints at it, *Proceedings Soc. Antiq. Scot.* 1897-8, p. 363.



PLATE I



THE ALPHABETIC INSCRIPTION ON THE NEWTON STONE.

PLATE II



THE NEWTON STONE: FRONT VIEW



PLATE III



THE NEWTON STONE: SIDE VIEW





## THE NEWTON STONE

inscriptions (call them **A** and **B**) which, though intermingled, are clearly contrasted. **A** consists of lines 1, 2, 5; **B** of lines 3, 4, 6.<sup>2</sup> [See the diagram page 392, where the two inscriptions are separated, and check this description by reference to PLATE 1]. The latter three are well aligned, in a square block of writing, with letters closely compacted together, and containing numerous Greek-looking characters, though the inscription is obviously not Greek. The former are straggling, with sprawling characters, and there is no attempt at aligning them with the **B** group or among themselves. The **A** group contains no Greek-like letters except **Y**, which hardly counts, as this may belong to any one of several alphabets.

Since about 1892 it has been generally agreed that the first two lines of the alphabetic inscription correspond to the beginning of the Ogham; but I do not think that it has been noticed that the transliteration is continued in line 5 (line 3 of inscription **A**). Inscription **A** is thus a transliteration of the Ogham. But the transliteration is so faulty that it could not have been made by anyone who had the least understanding of what the Ogham meant; and therefore it could not be one half of a contemporary bilingual. It is just what would be produced by an amateur, with no knowledge beyond what he could gather from the Ogham alphabet, as published in 1781 in Vallancey's *Grammar of the Ibero-Celtic Language*. The letters in which the transliteration is expressed are recognizable with the help of the Ogham original, except for a few ambiguities. They are not like any ancient forms of Roman letters; but they bear a family resemblance to some of the queer perversions of inscriptions to be found in, let us say, early editions of Camden, or in Rowlands' *Mona Antiqua Restaurata*.

I am not at present concerned with the interpretation of the Ogham, beyond a few details which it is here necessary to mention. It is to be read **IDDARRNNN VORRENN IPUOR** in the first line: I leave the second line alone for the moment. I shall content myself here with saying that I take the first word to be the same as **EDDARNONN**, which we find (with some variety of spelling) on three other stones, and which I would interpret conjecturally as meaning 'his stone', 'his grave', or the like: **VORRENN** I regard as a personal name, **VORR**, with a postfix having a genitive sense; and **IPUOR** is a noun, meaning some kind of

<sup>2</sup> While giving this paper its final revision, and after I had drawn the diagram (FIG. 2), I noticed (as explained below) that the third line of inscription **B** properly belongs to inscription **A**. It seems to be an afterthought, which will to some extent account for its conformity in outward appearance to inscription **B**.

## ANTIQUITY

relationship—'son', 'nephew', or the like—with a dative postfix. To a justification of these interpretations I shall return on another occasion; it would require a complete marshalling of all the epigraphic material surviving in Pictland.

When I drafted this paper, I supposed that the compiler of the inscription, puzzled by the three Ogham letters following the X (*i.e.* the P in IPUOR), had omitted them. These letters are very carelessly set out, without due regard to the differentiation between vowels and consonants: it is probable that they could not be accurately read even now, if we had not other inscriptions for comparison. But at the last moment I realized that the transliteration is continued on what I had already drawn out as the third line of inscription B. The λ-shaped character represents the very straggling 'P' of the Ogham, and the *opo* following (as we may represent them for typographical convenience) is the transliterator's effort at the UOR.<sup>3</sup> The whole of his transliteration is thus

Line 1, EDDE (or ETTE)  
 „ 2, ECNUNFAUR  
 „ 5, RANNI  
 „ 6, λORO

Remembering that Vallancey, like all other Oghamists of prephilological days, rendered the Ogham v by F; that the carver of the Ogham has been beyond measure careless in the setting of his scores on the stem-line; and that the transliterator could not possibly have had the faintest notion of the meaning of the text; we must admit that he comes not uncreditably out of his struggle with a peculiarly difficult inscription. Thus, the first two Ogham R's are cut in such a way that there is some excuse for the transliterator having read them as E, C. On the other hand he should not have counted only four scores in each of these letters, where there are really five; but similar mistakes have been made by Oghamists much more highly experienced than he was. Incidentally, this is the first attempt ever made in modern times at deciphering an ancient Ogham inscription: some wild renderings of the inscription on Mount Callan, co. Clare, published in 1785, do not count, as we now know that this inscription is another modern 'folly'.

But when all is said and done, we must repeat, that there are too

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<sup>3</sup> These four letters have the appearance of having been squeezed in where we find them after the rest of the inscription (including the five letters which follow them) had been cut. Apparently the author of the alphabetic inscription returned to this difficult part of the text after the rest of his work was finished.



## THE NEWTON STONE

many blunders in the transliteration in inscription A to allow us to accept it as a contemporary echo of the Ogham. This being, I hope, established, we now turn to the much more difficult problem of explaining inscription B.

*The Gentleman's Magazine*, the medium through which the 'Edward Hudsons' of the time were kept in touch with the progress of discovery and invention, announced, in its supplement for 1801, p. 1194, the discovery of the Rosetta stone, in the following words:—

General Dugun, lately returned from the Egyptian expedition, brought home two copies of a remarkable inscription found on a piece of black and extremely fine-grained granite. The inscription is three-fold: one portion presents a succession of hieroglyphicks in several very regular lines; another portion, which has not yet been sufficiently examined, presents a greater number of lines, in characters which yet leave some uncertainty, and which require a very attentive examination; the remaining portion consists of 53 lines in Greek.

Particulars are then given as to the steps being taken for its decipherment.

In August 1802, p. 725, the same journal published a letter signed 'D.H.', describing how the stone, 'dug up by the French' had been, 'with other antique fragments, made by capitulation the property of the British nation'. But, the writer goes on to complain, 'copies had been previously taken of it by its former possessors, who, with their accustomed vivacity, have anticipated us in the attempt to illustrate (*sic*, read 'elucidate'?) it'. He then gives details of the preliminary efforts at decipherment by Silvester de Sacy, adding: 'That the three inscriptions commemorate the same thing is clear from the words at the close of the Greek, ordering it to be engraven on hard stone in *sacred*, *native*, and *Greek* letters'. This sentence I take to be the inspiration of the local 'Edward Hudson'.<sup>4</sup> He had observed the Oghams on the edge; had deciphered them to the best of his ability, presumably with the aid of Vallancey's publication; and then, fired with the desire to possess a 'Rosetta stone' of his own, he added to the inscription, which he regarded as being in sacred (druidic) characters, a 'translation', in 'native' and in 'Greek' letters—respectively the inscriptions here called A and B.

*The Gentleman's Magazine* continued to interest its readers on the subject of the Rosetta stone. In December 1802, p. 1106, it published a translation of the Greek inscription. On p. 1127 of the same issue, in a review of Wilson's *History of the British Expedition to Egypt*, the

<sup>4</sup> For convenience of reference I designate the unknown person responsible for the Newton inscription by the name of his Rathfarnham counterpart—thereby avoiding the harsh word 'forger'. The transaction was a 'folly' rather than a fraud.

## ANTIQUITY

monument is mentioned as 'a stone of black granite with three inscriptions, hieroglyphic, Coptic, and Greek'. 'D.H.' had already mentioned the difficulty which Silvester de Sacy was finding in his work of interpretation, owing to his imperfect knowledge of *Coptic*. These two references might suggest that 'Hudson', to make his inscription conform more closely to the Rosetta model, had picked out a stray sentence of Coptic from some book and had made an amateurish reproduction of it. This occurred to me as a possible explanation of the Greekish-looking letters in a non-Greek inscription. But Mr W. E. Crum, who most kindly allowed me to consult him, told me that he could see nothing in it suggestive of Coptic.

We must therefore conclude that inscription B is mere childish galimathias, a meaningless mixture of Greek and arbitrary characters.

Let us now enquire how this accords with the evidence. Lord Aberdeen, writing from memory in 1855, *thinks* that he first saw the stone in 1804, and that shepherd boys had discovered it in the preceding year. These dates are probably correct. His lordship returned from his travels in Greece in 1803, so that his visit could not have been earlier. He had reason to remember the year 1804, as it was the year of his graduation (Dict. Nat. Biog.). He goes on to say that the stone was then covered with hard grey lichen, which made the letters 'scarcely distinguishable': though he adds that, at the date of his letter in 1855, 'in consequence of the frequent tracings and rubbings off to which the letters have been submitted, they have now entirely lost their natural surface'. But rubbing, however frequent, would not disturb hard lichen; and it cannot be said that the surface of the stone shows signs of injury from this or any other cause. Lord Aberdeen was a traveller and a classical scholar; he had first-hand experience in, at least, *looking at* ancient inscriptions; and yet he passed over as 'scarcely distinguishable' an inscription of which, three years later, an anonymous writer could publish a most accurate and complete facsimile. Here is a fact that surely calls for explanation; and I submit that a satisfactory explanation will be found in the following reconstruction of the course of events.

(1) The shepherd boys discovered the *stone* (not the *inscription*) about 1803. They noticed certain superficial marks in the lichen which they took for writing. (They were probably illiterate.)

(2) The story of a written stone got about, and Lord Aberdeen visited it in 1804. He came predisposed to find an inscription, and saw some 'scarcely distinguishable' marks, of which he could make



## THE NEWTON STONE

nothing. Anyone who has tried to read a worn inscription in the obscure light of a thicket of trees will be ready to make every allowance for him. Writing long afterwards, when the inscription had become familiar, and with no suspicion as to its genuineness, he felt sure that it must have been there all the time, but that the circumstances prevented him from seeing it properly: his later knowledge (a too frequent experience of all of us) having coloured the actuality of his recollections. But, I submit, *the inscription was not there at all*, when Lord Aberdeen saw the stone in 1804.

(3) The unknown 'Hudson' also visited the stone, about the same time; and he first noticed, and essayed to decipher, the Ogham.

(4) *The Gentleman's Magazine* had published an account of the Rosetta stone in 1802. 'Hudson' was thereby inspired to perpetrate his 'folly', which he must have done very shortly after Lord Aberdeen's visit. The rumour that the stone was already inscribed may have encouraged him to improve thus upon the vague handiwork of Nature.

(5) 'R' published the forged inscription in *The Gentleman's Magazine* in 1807. The remarkable accuracy of 'R's' copy suggests a suspicion that 'R' and 'Hudson' might have been the same person; but against this is the fact that 'R' makes no mention of the Ogham, of which 'Hudson' must necessarily have been aware. Exactly what 'Hudson' gained or expected to gain by the transaction does not fully appear. He may have died soon after the inscription was cut, thereby losing whatever profit or kudos he had anticipated; other discoverers (including 'R') may have 'butted in' before he had judged the time ripe to spring the discovery on the world; or perhaps he may have merely intended to 'lie low' from the first, and to enjoy the private fun of watching the disputes of men of learning over his handiwork.

The second line of Ogham writing reads IOSIR. This is not what it should have been: we want the personal name of the owner of the monument, with the Pictish dative postfix *or* or *vor*. And such a name was there originally. The termination *-or* can be faintly traced, just above the final R of IOSIR. It can be detected in the left-hand photograph of Romilly Allen's *Early Christian Monuments of Scotland*, III, 197 (fig. 214); and relics of it are very clear on the excellent cast of the stone in Edinburgh Museum. Owing to adverse conditions of lighting, on the day when I visited the stone itself, I failed to observe them on the original monument.

Now General Vallancey had just published in *Collectanea de Rebus hibernicis* (v, 90), a bronze-age gold ring with expanding cup-like

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terminals—one of the commonest of Irish gold ornaments—upon which someone had scratched certain marks. The ring, which I have had an opportunity of examining, is genuine; but the marks are not much older than Vallancey's own time, and may conceivably have been made for a practical joke upon that great man himself. On the one expansion is the word UOSER in Ogham letters: on the other are four meaningless characters, which Vallancey had no difficulty in recognizing—and reading—as 'Phoenician or Estrangelo'. With these inscriptions as his text, he indulges in a page or two of characteristic philological delirium, in which Aesar, Dusares, the Arabic Al-Zuhari, and, of course, Osiris, are all mixed up together in a sort of mythological Irish stew.<sup>6</sup>

To our 'Hudson' who (on the theory here propounded) had Egypt in his mind, 'Osiris' would come as a flash of inspiration: and he seems to have instructed his mason to batter out the original second line of the Ogham—the pock-marks remain still, as a record of the crime—and to substitute an invocation of that deity.

The two lines of Ogham writing look very different. The second is weakly cut, amateurish and straggling. A stem-line is provided, though as the photographs will show, a ridge was there already, like the ridge on which the longer line is written; the latter shows no trace of a stem-line. I find it impossible to believe that the two Ogham lines could have been the work of the same hand.

The last five letters of the alphabetic inscription appear to be the transliteration of this addition to the Ogham; and as has already been remarked, they seem to have been cut before the four letters which precede them. Vallancey's ring reads UOSER, the Ogham IOSIR. 'Hudson' or his stonemason collaborator was careless about Ogham vowels, but in his transliteration he approximates to the Vallancey form: the letters look like the word disguised as YΩSIR. The photographs would suggest that the penultimate letter is E (as it ought to be): but the wavy appearance of the upright stroke is due to some slight flaws in the surface of the stone.

So much, then, for the inscription with which the Newton stone has been wasting time, energy, ingenuity, and journey-money for over a century. May it now follow the ragged regiment of spectres to which it has given birth, into the oubliettes of Tartarus: and may we hear no more of it!

<sup>6</sup> An easily accessible copy of Vallancey's cut of this object will be found in Brash's book on Ogham inscriptions, plate XLI.



## Blood-groups and Race\*

by J. MILLOT

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IT has been known for a long time that there might exist differences in the blood of animals of different species: morphological differences connected with the shape and size of the red corpuscles, or chemical differences relating to variations in the respiratory pigment, called haemoglobin. It was also known that individual differences, acquired during life, might appear in the blood: as a result of illness, for instance, certain properties of the blood might be permanently modified by the formation and accumulation of antibodies. At the beginning of this century it was discovered that beside these variations of a zoological or accidental nature, human blood possesses hereditary constitutional differences of an individual and racial importance. The study of these, and especially of their anthropological application, has advanced considerably; at the present moment it almost forms an independent branch of science. The investigations bearing on it are to be counted by thousands, and several scientific periodicals are now exclusively devoted to it.

What are these blood-differences and how were they discovered? Their history is closely associated with blood-transfusion.

Blood is known to consist of a large number of small corpuscles in suspension in a liquid, the plasma. The corpuscles are of two sorts, white (called leucocytes) and red (called erythrocytes) which are much more numerous. Two constituents may also be distinguished in the plasma: a liquid (serum) consisting essentially of a 7 per cent. solution of salt water, and of albuminoid substances in solution, most of which are precipitated if the blood flows out from the organism, producing the well-known phenomenon of coagulation. Under normal conditions the red corpuscles float freely in the plasma, without adhering to each

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\* Translated by the Editor, who wishes to thank Professor Ruggles Gates for reading the article and correcting some technical errors of translation.

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other ; but under certain conditions adherence occurs and the erythrocytes collect together in bunches, producing the phenomenon of agglutination. This result can happen both *in vitro*, that is to say, in blood that is kept in glass vessels, and *in vivo*, in a living organism ; but their circulation is seriously impeded and very grave accidents may be produced.

The idea of blood-transfusion, of injecting foreign blood into a subject weakened by excessive bleeding, is not a new one ; we know of undoubted instances occurring as early as the 15th century : that of Pope Innocent VIII, for example. The results of these early transfusions, made mostly with the blood of heifers or lambs, were so disastrous that in the 17th century the operation was made illegal by an act of parliament passed in Paris. The cause of these failures was partly discovered in 1874, when Landois showed that the serum of an animal of one species agglutinates the red corpuscles of animals of other species ; that is to say, that if one mixes the blood of two animals of different species, there ensues a general clumping of their corpuscles : that has been called hetero-agglutination. At that date it was thought that agglutination only took place between the bloods of different species. Accordingly transfusions were then made again but only with human blood ; the results were good in certain cases but still disastrous in others. That was the state of affairs when, about 1900, several doctors observed fortuitously that the serum of certain sick people agglutinated the red corpuscles of healthy patients. This phenomenon, which was called iso-agglutination, was at first regarded as indicating a pathological condition ; but researches were continued and Landsteiner proved that, when the blood of two perfectly healthy people was mixed, agglutination was produced or not according to the patients who were the subject of the experiment. He concluded from his experiments, on the one hand that iso-agglutination can exist in a normal human being, on the other hand that the iso-agglutinating properties are not the same in all human beings, and that these should, from this point of view, be divided into several categories or groups. That is the double discovery of fundamental importance for which Landsteiner was awarded the Nobel prize—a discovery whose origin is to be found in the great volume of research devoted for a quarter of a century to the serological properties of the blood.

How should the facts brought to light by Landsteiner be interpreted ? The simplest explanation, and the one most usually given at the present moment, is that iso-agglutination results from the reciprocal







TWO DROPS OF BLOOD, THE ONE ON THE LEFT NORMAL, THAT ON  
THE RIGHT AGGLUTINATED



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action of two kinds of substances whose chemical nature, however, remains completely unknown: the one called agglutino-gen is contained in the red corpuscles, the other, or agglutinin is in the serum. Analysis has shown that there are at least two different agglutinogens, conventionally called A and B, and capable of being present in the red corpuscles either singly (A or B) or together (AB), or not at all (O): certain individuals therefore have only A, and others have B, and others both agglutinogens; while yet others are totally devoid of either. It is this which is described somewhat inaccurately (but which has found its way into scientific parlance) as the existence in the human species of four blood-groups—group A, group B, group AB, and group O, this last containing neither A nor B.

To the agglutinogens of the corpuscles correspond agglutinins in the serum which are distinguished by the Greek letters  $\alpha$  (or anti-A) and  $\beta$  (or anti-B). The same blood never possesses agglutinins active in the presence of their opposites; or in other words, the serum of a given individual never agglutinates its own corpuscles—a result which in fact would make circulation, and consequently life, impossible. Accordingly the serum of bloods belonging to group A contains, not agglutinin  $\alpha$  which agglutinates A, but agglutinin  $\beta$ ; and it is the serum of group B which contains agglutinin  $\alpha$ . The group AB, possessor of two agglutinogens, can never have agglutinins, whilst group O possesses at the same time both  $\alpha$  and  $\beta$ . From this it follows that the complete formulas of the blood-groups are  $A\beta$ ,  $B\alpha$ ,  $ABO$ ,  $O\alpha\beta$ . This nomenclature is the one usually adopted, but two others, those of Jansky and Moss, are also in use.<sup>2</sup>

If one brings together the blood of two people belonging to the same group there is no result—the blood mixes normally. If on the other hand one brings together blood samples from groups A and B,  $\alpha$  will react with A,  $\beta$  with B, and agglutination will ensue.<sup>3</sup>

In the light of these facts we now know how to practise transfusion without risk of accident; it is enough to establish to which group the

<sup>1</sup> In this deliberately simplified description, we are not taking into account either agglutinogens or accessory agglutinins.

<sup>2</sup> The groups are indicated by Roman figures; according to Jansky  $O=I$ ,  $A=II$ ,  $B=III$ ,  $AB=IV$ ; according to Moss  $AB=I$ ,  $A=II$ ,  $B=III$ ,  $O=IV$ .

<sup>3</sup> The hypothesis of agglutinins and agglutinogens is a convenient explanation of the facts; but does it correspond with reality? Are there definite chemical bodies involved? No doubt we have to reckon only with special properties which may be common to many different substances, under certain well-defined physical conditions (Mendes—Correa).



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patient belongs, and to obtain the blood from a donor belonging to the same group as himself. Nothing is easier, for in the big towns there have come into existence specialized institutes whence, by telephoning, one can immediately obtain a 'donor' of the required type. In actual fact, the agglutinins are only active when concentrated, and those of the donor, diluted in the blood of the receiver, appear practically without effect: it results that one can always transfuse blood of group O (universal donor) into a wounded patient, whatever his group, without serious results; and that, conversely, a subject of group AB can receive without harm any blood whatever (universal receiver).

Human beings, therefore, are divided into four groups, according to the serological properties of their blood. These preliminary explanations having been given, let us see what main characteristics are revealed by the reactions of agglutination; after which we will consider the applications of the results to different subjects, particularly those of an anthropological nature.

It is a fundamental fact that blood-properties have a remarkable permanence. Their stability may even be regarded as absolute; a man never changes his group under any circumstances whatever. We already have observations covering 25 years, and we know that age never modifies the reactions, nor does any physiological condition. Illness has no effect, nor have treatments of the most varied kind, in spite of what may have been said to the contrary. X-rays, like those of radium, as repeated anaesthesia, produce no effect. Lastly, the group is not changed even after a transfusion from the blood of another group; and the case is actually recorded of a subject of group AB (universal receiver) having undergone 75 transfusions with blood from each of the four groups without suffering any modification of his own blood.

A second characteristic of blood-properties is the precocious manner in which they are differentiated. The agglutinogens appear before the agglutinins. They are sometimes discoverable in the second month of embryonic life. One can in any case always detect them during the last weeks preceding birth, and so plainly that it is always possible, normally, to know to what group a new-born child belongs.

If blood-properties are fixed and precocious, they do not manifest the same degree of strength in all subjects. Agglutinins, like agglutinogens, differ in strength according to individuals, race and age. It has been recorded, for example, that the power of agglutinins, always very weak at birth, increased up to thirty years of age, to decrease after forty; and that it was greater amongst Malays than amongst Europeans.



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The fact of belonging to a given group is a hereditary characteristic. It would be impossible here to give even a rapid outline of the mechanism of heredity. What is essential is to know that in a child no agglutinin can appear which is not present in the blood of one or other of its parents. If the father and mother belong to group A, all the children may belong to group A; if they belong to group O, all the children will belong to group O; if the parents do not belong to the same group—if, for example, one is A and the other B—the children will differ and be A, B, AB or O.<sup>4</sup> Recognition of these facts has resulted in curious applications of them. It has made it possible, for instance, to restore to their respective mothers newly-born infants which had been mixed up immediately after birth. Above all it has been used in medico-legal practice to investigate paternity.

Attempts have been made to establish a connexion between blood-properties and other morphological or physiological characters of the organism, such as stature, skull-form, pigmentation, hair, age of onset of puberty, etc. All these attempts appear to have failed, the different correlations announced having all been more or less falsified by the results, in the light of more extended observations. Thus, it had been stated that in Sweden the majority of group B subjects were brachycephalic; but this has not been generally established, and it may be regarded as a mere coincidence. Some doctors believed they could detect a relationship between the serological properties of blood and the liability to certain maladies, such as infectious fevers, cancer, etc. Victims of tuberculosis, for instance, belonged most frequently to group A, whilst members of group B suffered from a noticeable weakness of the nervous system, and amongst them psychoses were more often observed and criminals most common. But existing observations are not nearly numerous enough to justify such conclusions.

Lastly there is the important belief that the reactions of iso-agglutination are not peculiar to the human species. They have been found in several other mammals. In most they appear to be much weaker and less constant than in man, and to result from different agglutinogens; on the other hand, human agglutinogens and agglutinins are also found in the anthropoid apes, Gorillas, Chimpanzees, Orang-utangs and Gibbons. It follows that one could transfuse the

<sup>4</sup> It may be added that according to Bernstein (1925) and Snyder (1925) blood-groups inherit as three multiple allelomorphs corresponding to the agglutinogens, A and B being dominant in association with a similar recessive R, and not as two independent pairs of factors, as was formerly thought.

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blood of a chimpanzee into a man of the same group without ill effect, whilst transfusion of human group B blood would have serious consequences.

These few general observations are enough to demonstrate the great interest, from many points of view, of agglutinative phenomena and the researches which they have inspired. We shall next see that a knowledge of this subject is of the greatest importance for anthropologists, and that the fact of belonging to a given group—a fact, as we have seen, independent of age, sex and every other physiological condition—seems, on the other hand, to be a very definite function of race.

It had been noticed ever since the first investigations, that the proportions of the different groups maintained a remarkable constancy in a given population, amongst the Germans or the Italians, for instance. But the suggestion (due to MM. L. and H. Hirszfeld) that there might be a relation between the anthropological content and the distribution of agglutinogens was not put forward until later. Being attached during the war to the medical service of the Army of the East, these biologists had opportunities of examining the serological properties of the blood of a large number of soldiers or civilians belonging to very different races. They established three categories: one marked by a high percentage of subjects of group A and a low percentage of B, and including the majority of European races (European type): a second showing on the contrary a high percentage of B and a low one of A, comprising Mongoloids and Ethiopians (Asio-African type): and a last category containing approximately equal quantities of A and B, comprising Russians, Turks, Arabs and Jews (intermediate type). This discovery, published in 1919, gave rise to a considerable number of investigations, producing an enormous mass of documents of varying merit, and emphasized the great ethnological interest of blood-groups. It would need many pages to give a complete account of the results, and we must therefore confine ourselves here to describing a few of the essential features.

Hirszfeld thought he could conveniently synthesize these features by calculating for each race a 'biochemical index', obtained by dividing the percentage of A by the percentage of B, that is to say, by establishing the proportionate fraction  $\frac{A+AB}{B+AB}$ . If out of 100 subjects of a race one found 40 A, 10 B and 5 AB, the index would be  $\frac{40+5}{10+5} = \frac{45}{15} = 3$ . The biochemical index for the European type thus fell between 2.5 and 4.5,



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the index of the Asio-African type between 0.5 and 1.1, and the index of the intermediate type between 1.3 and 1.8

This index has been much used. But the most recent researches have shown, on the one hand that the sero-ethnic types established by Hirszfeld were not exact enough, on the other that the biochemical index was open to serious criticism; in actual fact, all races in which the proportion of A is equal to that of B, whatever might be their absolute value, would have a similar index, equal to 1, whatever the number of the O's or of the AB's, which is obviously unsatisfactory.

Many other formulae have been proposed to replace that of Hirszfeld, but without offering any clear advantages over it. On the other hand successful use has been made of tables, which have proved most instructive.

The examination of such tables enables us, following Ottenberg, to distinguish a certain number of quite well marked sero-ethnic types. One such is remarkable for its extreme poverty in both A and B, that is to say it includes almost exclusively only the subjects of Group O. It comprises the American Indians, the Filipinos, and most of the Eskimos of pure race; it is called the Americo-Pacific type.

A second type, adjacent, rather deficient in B, but richer in A, is represented by the Australians (Australian type). A third, again poor in B, but very rich in A, includes all western European type. A fourth type is marked by a moderate proportion of A and B, and includes the Negroes, Melanesians, southern Asiatics (Annamites, Javanese) and is called the Afro-Malay or Afro-south-Asiatic type. A fifth, embracing the Near Easterns (Turks, Persians, Armenians, Arabs) as well as Russians and Czechs, has a moderate proportion of B and a high percentage of A (intermediate type). A sixth has a very high proportion of A with a considerable but lower proportion of B; called Hunan, after the name of the Chinese province, it includes, besides that region, the Japanese, a part of the Koreans, and in Europe the Poles, Ukrainians and Hungarians. Lastly, a seventh type, poor in A, but the richest of all in B, comprises all the Hindus, the north Chinese, the Manchurians, and in Europe the Gipsies (Indo-Manchurian type).

Such in outline is the actual classification of races according to the serological properties of their blood. Some anthropologists have criticized it severely, going even so far as to deny that blood-groups have any ethnological value at all. They claim that from a study of it one can deduce no valid argument bearing on the relationship of races. They point out, for instance, that Hindus and Europeans have



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wholly dissimilar blood, whereas there are good reasons for supposing that both are descended from a common stock ; on the other hand, the Lapps, who are of Asiatic origin and very different from the Norwegians, are made to belong with them to the European type. They argue also that Jews occur in almost all the categories, as much amongst Afro-Malays as in Europe (German Jews), amongst the Intermediates (Spanish Jews) or amongst those of the Hunan type (Roumanian Jews and Jews of Beirut). These objections have very little weight. It is now agreed that Hindus and Europeans are much less closely connected than was formerly supposed, on the strength of linguistic evidence wrongly interpreted. The Lapps have very irregular blood-formulae, and they are sufficiently intermarried with their Scandinavian neighbours to make it not at all surprising that their original blood should have been modified and now approximates to that of Europeans. Finally, we may now regard it as certain that the Jews do not constitute a true race, but a group of communities which are ethnically distinct and united primarily by a common bond of religion. It may be added that if Grove, for instance, found very different blood-formulae amongst the different Ainu tribes, that was because he did not guard sufficiently against sources of error (consanguinity) which invalidate his statistics.

On the other hand, many accurate observations, as Snyder has emphasized, confirm the real ethnic value of the reactions of agglutination, and show that populations of different blood can live side by side for centuries in the same country without their serological properties undergoing any modification, if there is no intermarriage. Thus the index of the Japanese and that of the Ainu is very different, although these two races have been neighbours in Japan for several millennia. The facts are particularly striking in the United States ; the three great races which have lived there on the same territory for three hundred years still retain widely different indices, the Indians having one of 9.5, the whites 3.6 and the blacks 1.4. Hungary provides another choice example, because in that country are brought together colonies which are ethnically very different—Hungarians from the East, Gipsies, probably of Hindu origin, and Germans of a date before the 18th century ; the study of their blood shows, in the plainest possible fashion, that the Germans of Hungary react like those of Germany, whilst the Gipsies are of the same type as the Hindus, and that the Hungarians come close to the Turks—results agreeing entirely with what we know from other sources.

We may conclude that blood-properties provide us with a genuinely



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valuable means of revealing the purity of a race, and that they owe their importance to the fact that they are at once strictly hereditary and as independent as possible of environmental influences.

Certain anthropologists have gone further and tried to extract from serology information about the origin of races. The first theory of this kind is that of Hirszfeld, who was struck by observing that, as one passes from western Europe to eastern Asia, the proportion of A diminishes and that of B increases. The fact is indisputable. A passes from 45 per cent. amongst the Norwegians to 27 amongst the Manchus. In the case of B, the change is particularly striking; one meets in turn with 12 to 14 per cent. in western Europe—20 to 23 per cent. in the Balkans—25 per cent. in the Turks and Arabs—34 to 49 per cent. amongst the Indo-Chinese, Chinese and Hindus. Hirszfeld thought that from these facts he could infer a dual origin for the human race. There would have existed, according to his ideas, two human stocks, one having group A in its blood and coming from northern and western Europe, the other, with group B, coming from the east, perhaps from India. The two stocks having mingled in the regions where they came in contact, would have given birth to an intermediate type, the B element having, for instance, been introduced into Europe with the different oriental and Mongol invasions. This view is entirely hypothetical. And while most anthropologists at the moment believe that the agglutinogens A and B may well have come into existence separately and in different regions of the earth, they will not admit a dual origin for the human race. It is known, in fact, that entirely new morphological, physiological or chemical characteristics may, in conformity with the laws of heredity, appear quite suddenly in a species. The phenomenon is called mutation, and numerous instances have been recorded in animal-breeding. According to Bernstein and Snyder the serological properties of the blood might thus have appeared as a mutation, and the human species be derived from a single stem which originally lacked both agglutinogens and agglutinins. One of the principal arguments in favour of this view is that the living representatives of races regarded as pure but on the way to extinction have group O either exclusively or at least dominant. Thus out of 112 Navahos examined by Rife, 111 belonged to group O and only one to group A. It would seem then that we may regard this group O as primitive. Starting with this primary human blood, there would have become differentiated the agglutininogen A in Europe, then the agglutininogen B in Asia; doubtless there would also have been a supplementary mutation of A in the Far

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East, responsible for the Hunan group. The supposition that group A is older than group B rests on the fact that there are several races, such as the Australians, who have group A but not group B, whilst none are known to have group B without group A. According to this view, the fact that pure-blooded Indians are all of group O would prove that they became separated from the Mongolian peoples before the appearance of any blood-mutation; and the fact that group A but not group B occurs amongst the Australians would be evidence of their having been isolated from their stock during the period that elapsed between the two mutations.

These views are strongly in favour at the moment. Professor J. B. S. Haldane expounded them eloquently in a discourse before the Royal Institution some years ago.\* We must however face the fact that they are largely hypothetical, and personally I can hardly admit them. Why, for instance, should the Senegalese have 19 per cent. of B, when they do not appear to have any trace of Mongolian admixture? One is driven to assume an independent mutation of B in Africa, which merely complicates matters. But above all the fact that human agglutinins and agglutinogens are found amongst the anthropoid apes seems to us to have a very important bearing on the problem, and to show in the clearest possible way that blood-groups have existed for a very long time indeed. Their recent simultaneous origin in monkeys and men is, in fact, most improbable; is it not more natural to suppose that the common ancestors already possessed agglutinogens? We will confine ourselves to saying, finally, that if a character can appear by mutation, it can also disappear by the same process; and it should be remembered that the majority of the mutations observed in the course of animal-breeding are regressive, and consist in the loss of a structure or of a differentiation, not in the appearance of a new character. Is it not more satisfactory to regard the Redskins, for example, as Mongoloids who have lost agglutinin B than to suppose that they left Asia before the appearance of the mutation?

We may conclude that, while the study of the reaction of agglutination may not be able of itself to solve any anthropological problem, and while no complete racial classification can be founded upon it so that up to a point it may have betrayed the somewhat fantastic expectations aroused at the start, yet it does provide means of appraisal which are of great interest, complementing and checking those derived

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\* See Note at end, no. 1.



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from other sources. No serious anthropological enquiry can in future dispense with it.

Let us state, in conclusion, that the agglutinative properties are not peculiar to the blood. They are found precisely the same in the different secretions—milk, saliva, gastric juice, bile, sperm; they are even found in the minced tissue. They are a constitutional feature of the whole organism. They are obtained from the blood merely because they are easier to detect there.

### NOTE

The following articles in English deal with the same subject :—

1. Prehistory in the light of genetics; a discourse delivered at the Royal Institution on Friday, February 20, 1931. Printed in 'The Inequality of Man, and other essays', by J. B. S. Haldane. London, Chatto and Windus, 1932, pp. 63-77 (blood-groups, pp. 63-70).
2. Blood-groups and physiognomy of British Columbia coastal Indians, by R. Ruggles Gates and George E. Darby. *Journ. R. Anthr. Inst.* 1934, LXIV, 23-44.
3. Eskimo blood-groups and physiognomy, by R. Ruggles Gates. *Man*, 1935, no. 36.
4. Gates. *Heredity in Man*, chapter ix. (Constable, London).
5. The distribution of the blood-groups, by Allison Davis. *Sociological Review*, 1935, xxvii; January, pp. 19-34; April, 183-200 (with list of books cited).
6. The relation of blood-groups to race and some particulars of the Southwest Pacific, by H. J. T. Bijlmer. *Journ. R. Anthr. Inst.* 1935, LXV, 123-31.

## The Site of the Palace of Odysseus

by W. A. HEURTLEY

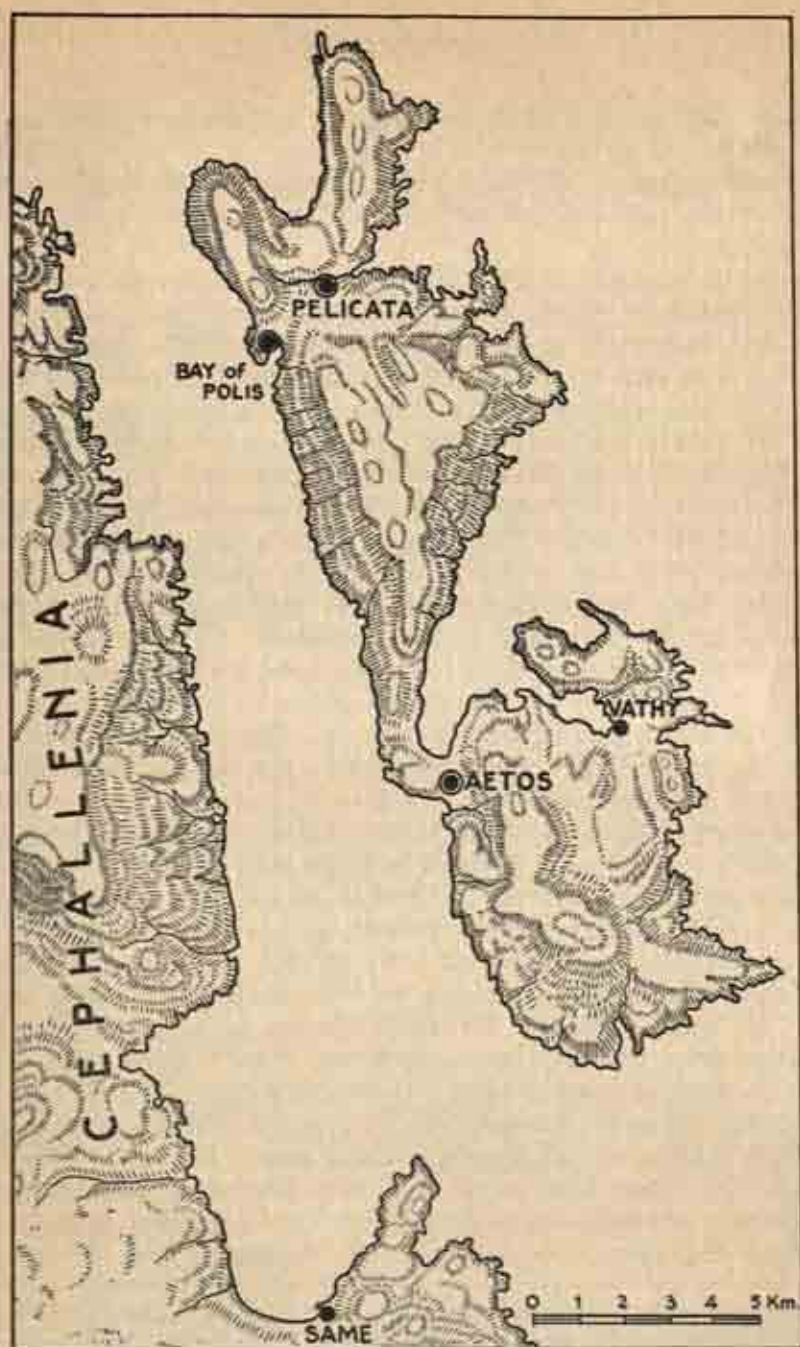
MANY Homeric scholars who believe that modern Ithaca is the Ithaca of the Odyssey have selected the hill of Pelicáta (FIG. 1) in the north of the island for the site of the palace of Odysseus.<sup>1</sup> Apart from considerations of Homeric topography, it is, in fact, the one spot in Ithaca which, on the analogy of Mycenaean sites elsewhere, strikes the eye immediately as suitable for a Mycenaean prince's citadel. It is sufficiently high; from its summit a ship entering any of the three harbours at the north end of the island can be seen; yet it is sufficiently distant from them all to be secure from any sudden attack by pirates.

The appearance of the hill (FIG. 5) must have been different in antiquity from what it is today, for its features have been changed by physical causes and the hand of man. For the effect of denudation there is good evidence towards the north of the hill. Here a rock-cut oblong grave, which must originally have been below the surface of the ground has been left, owing to the washing away of the soil, two metres above the present ground-level (FIG. 9).<sup>2</sup> But man has done most to change the aspect of Pelicáta, of which the barren slopes have been converted in course of time into a series of terraces for cultivating the vines. Terracing involves, as is known, the double

<sup>1</sup> Lord Rennell, to whose interest the excavations have been due, thinks that the city lay at Pelicáta, but the palace at the site of Hagios Athanasios, more than ten minutes distant (fig. 5). For various reasons I cannot agree, and chiefly because in Mycenaean times palace and town were closely united, in conformity with the feudal character of Mycenaean society. At Hagios Athanasios there is nothing demonstrably Mycenaean above ground, and our trial-pits revealed only three Mycenaean kylix-stems among a mass of Hellenistic and later pottery. The remains are best explained as those of a sanctuary, raised in honour of some god or hero, whose cult may have originated there in Mycenaean times. For Lord Rennell's views, cf. *Annual of the British School at Athens*, XXXIII, p. 1 sqq.

<sup>2</sup> Cf. Partsch, *Kephallenia u. Ithaka*, p. 60.





● • Excavations

FIG. 1. ITHACA

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process of cutting away and levelling up; the material obtained by cutting away (and anything else that happens to be handy) being used for the levelling up. The filling has, of course, to be kept in place by a wall of stones, and for this wall any stones which are lying about are requisitioned.

Thus the summit of Pelicáta hill appears today as a fairly level space, the limits of which coincide roughly with the contour line 150 (FIG. 2) but this level space is not entirely natural, being in places (FIG. 2, areas IV and VI) composed of filling held in place by terrace-walls. The composition of this filling is interesting (FIGS. 3, 4). It consists of stones, unworked and small, mixed with which are bits of clay bearing the impress of reeds, sherds and domestic objects such as mill-stones, pivot stones, mortars, stone-axes and clay spindle-whorls—evidently remains from houses with rubble walls and thatched roof plastered with clay, which stood on the summit and of which the debris, after they had collapsed, was shovelled down the slope, the terrace-wall having been first built to retain it. The thickness of the filling varies of course according to the formation of the ground. Above the stones is dark soil of an average thickness of 50 cm. in area IV and of about one and a half metres in area VI, also containing sherds and domestic objects. This soil may represent the successive house-floors which would be shovelled in after and on top of the rubble-walls, and which, as being composed of the accumulated rubbish of a long period of occupation would make suitable soil for cultivation.<sup>3</sup>

All the pottery from areas I, II and III, as well as from the numerous trial-pits sunk elsewhere on the summit, and practically all from areas IV and VI was Early Helladic (FIG. 6), of the same character as that known from Early Helladic strata on the mainland. But in area IV seventy 'Minyan' (Middle Helladic) sherds were found, nearly all of the blue-grey colour characteristic of West Greek sites and of Thermos in Aetolia: some were fragments of high-handled cups with incised spirals below the handles (FIG. 7: 3, 4, 7). Twenty similar sherds were found in area VI, all from the earth above the stone filling, but associated with them here were also sixty Mycenaean sherds (FIG. 8) thirty of which are kylix-stems (FIG. 8: 5) such as belong to the middle of the Late Helladic III period, but are extremely rare in its latest

<sup>3</sup> Of the other areas cleared, III was rather similar to those described; in II and V rough cobble pavements lay just below the surface, I contained pithos-burials lying below collapsed house-walls. All, except V, which was lower, were within or just on contour-line 150 (fig. 2).



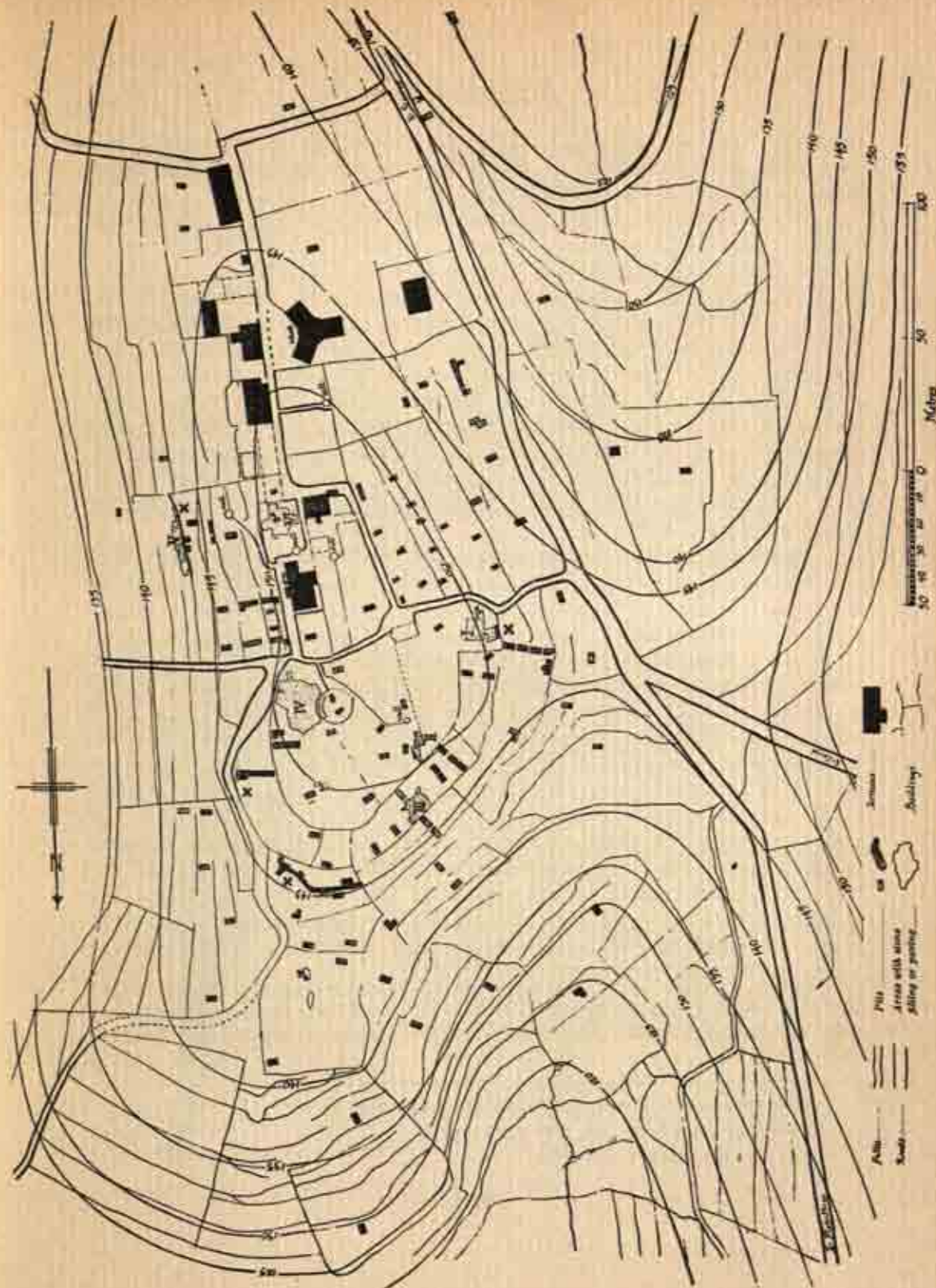


FIG. 2. FELICÁTA (CONTOURS IN METRES ABOVE SEA-LEVEL)

phase.<sup>4</sup> The low stems (FIG. 8: 1-4), are more difficult to place, but they resemble the stems of the kraters in the contemporary group from Lakkéthra in Cephallenia.<sup>5</sup>

The 'Minyan' and Late Helladic sherds are so scarce in comparison with the enormous quantity of Early Helladic, with which they were mixed, that it is impossible to believe that there was a pure 'Minyan' or Mycenaean settlement; and the inference may be drawn that in Ithaca, 'Minyan' and Mycenaean influences were only thinly spread over an earlier civilization which continued to survive. This problem does not however here much concern us (though I think that in Ithaca, as in Macedonia, an older civilization *did* persist), but what does concern us is that we have here the evidence from its pottery that a house was standing on the summit of the hill in the late Mycenaean period, but (what is especially to the point), that the occupation ceased about, or a little after the time at which, according to tradition, the Trojan war took place.<sup>6</sup>

In addition to the houses with rubble walls, there is evidence that houses built of better material also existed here. Scattered about on the top of the hill (FIG. 10) near area VI, and incorporated in rough walls at numerous points in its neighbourhood are many dressed blocks such as the modern builders of walls would hardly go to the trouble of making. The inference is that they found such blocks at hand and if so, there must have been some building, constructed of fairly good masonry, to which the blocks belonged. Now, in spite of the numerous trial pits which we opened on the hill on Pelicáta (FIG. 2) no pottery or objects of later date than the Mycenaean came to light, if we except those associated with a few graves, assignable to the Hellenistic age and all outside the circuit wall.<sup>7</sup> It is unlikely<sup>8</sup> that, if the summit had been occupied in Hellenistic times, Hellenistic pottery should not have been found there in the course of our excavations. Thus the building, whatever it was, appears to be not later than the Late Mycenaean period and since the stones in question are close to the area where the

<sup>4</sup> i.e., the 'granary' style phase (for which Gjerstadt has suggested the name LH IV), which the evidence from Cyprus and Palestine shows to belong to the twelfth century, rather than to the eleventh. Cf. *Quarterly of the Department of Antiquities in Palestine*, vol. v, p. 109.

<sup>5</sup> Cf. *Ἀρχαιολογικὴ Ἐφημερίς*, 1932, pl. v, especially 14.

<sup>6</sup> Cf. note 4. No 'granary' style pottery was found.

<sup>7</sup> Or its apparent line. Cf. next paragraph.

<sup>8</sup> But not, of course, impossible.



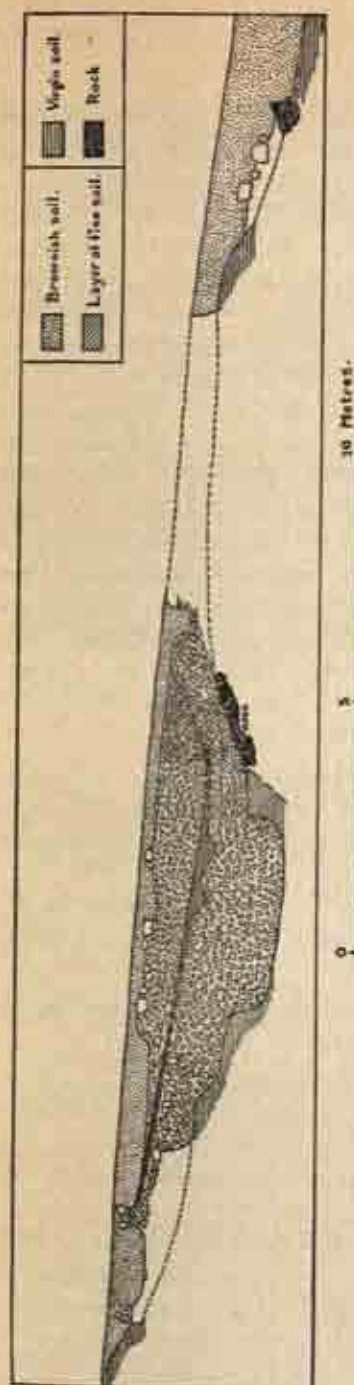


FIG. 3. AREA IV, SECTION EW

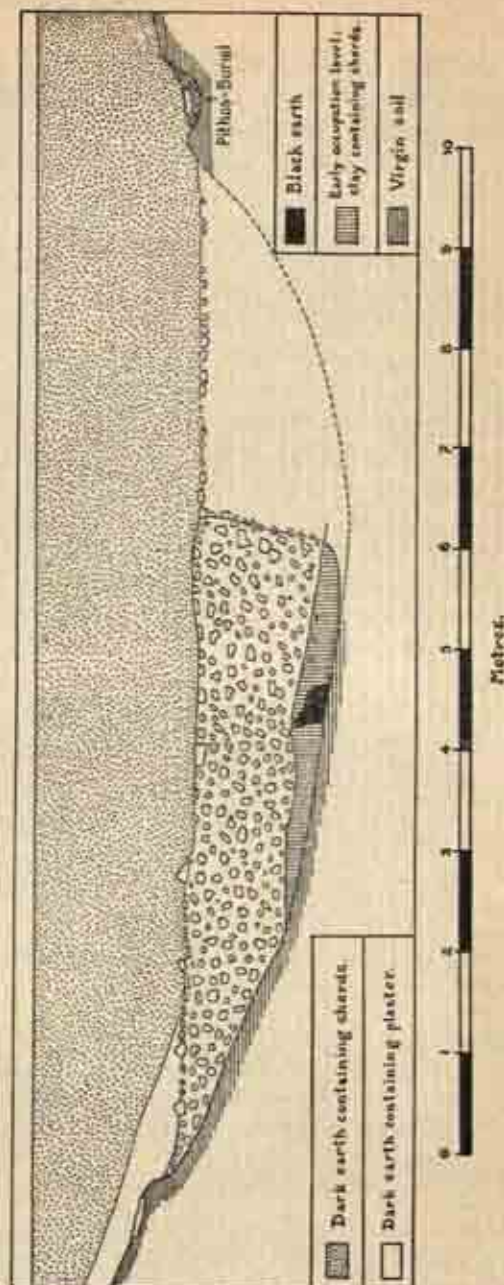


FIG. 4. AREA VI, SECTION EW

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Late Helladic III sherds were found, it is at least likely that sherds and building, to which the stones belonged, are contemporary. It is thus arguable that these stones are remains of a Mycenaean building.

In addition to the building or buildings of good masonry, a circuit wall with a foundation of roughly hewn blocks once enclosed the summit. The foundation is *in situ* in one place (FIG. 12) and the approximate line can be picked up in at least two places (FIG. 11)\* and elsewhere are many blocks which have been displaced from it, but which, on account of their size, cannot be far from their original position. Most are in the neighbourhood of contour line 145 (FIG. 2).<sup>10</sup> Though not enough of the wall is left to enable us to decide on purely architectural grounds to what age it belonged or what was the nature of the upper part, the absence of later pottery that can be associated with it, makes it unlikely that it is later than the L H III period, and on the analogy of the fortification-walls of Mycenae and Tiryns, may well belong to that period. Actually the pottery associated with the wall (*i.e.*, against, below and in one case, between the blocks), was Early Helladic, and, though this is not inconsistent, as has been seen, with an L H III date, may mean that it is earlier still. But in any case it can hardly be later.

To resume the evidence : we have at Pelicáta,

- (1) A site naturally adapted for a Mycenaean citadel, and one selected by many scholars, on topographical grounds, as the site of the palace of Odysseus.
- (2) Evidence of an extensive pre-Mycenaean settlement, which there is reason for thinking was continuously occupied until and during L H III times.
- (3) Late Helladic III sherds, belonging to the phase that preceded the traditional date of the Trojan war, and none later.
- (4) Traces of buildings of good masonry on the summit and of a circuit-wall just below it, neither of which seem to be later than that period and both of which may belong to it.

The cumulative value of all this evidence is respectable, and if we add that of the Mycenaean sherds (also 13th century), found by us at Stavros, and at the fountain of Asprosykiá, a short distance from it, and if we add that of the ex-voto plaque, bearing the inscription 'a vow to Odysseus', found in the cave-sanctuary at Polis, which shows that the

\* The places are marked x in FIG. 2.

<sup>10</sup> On the west, near contour-line 150.



PLATE I

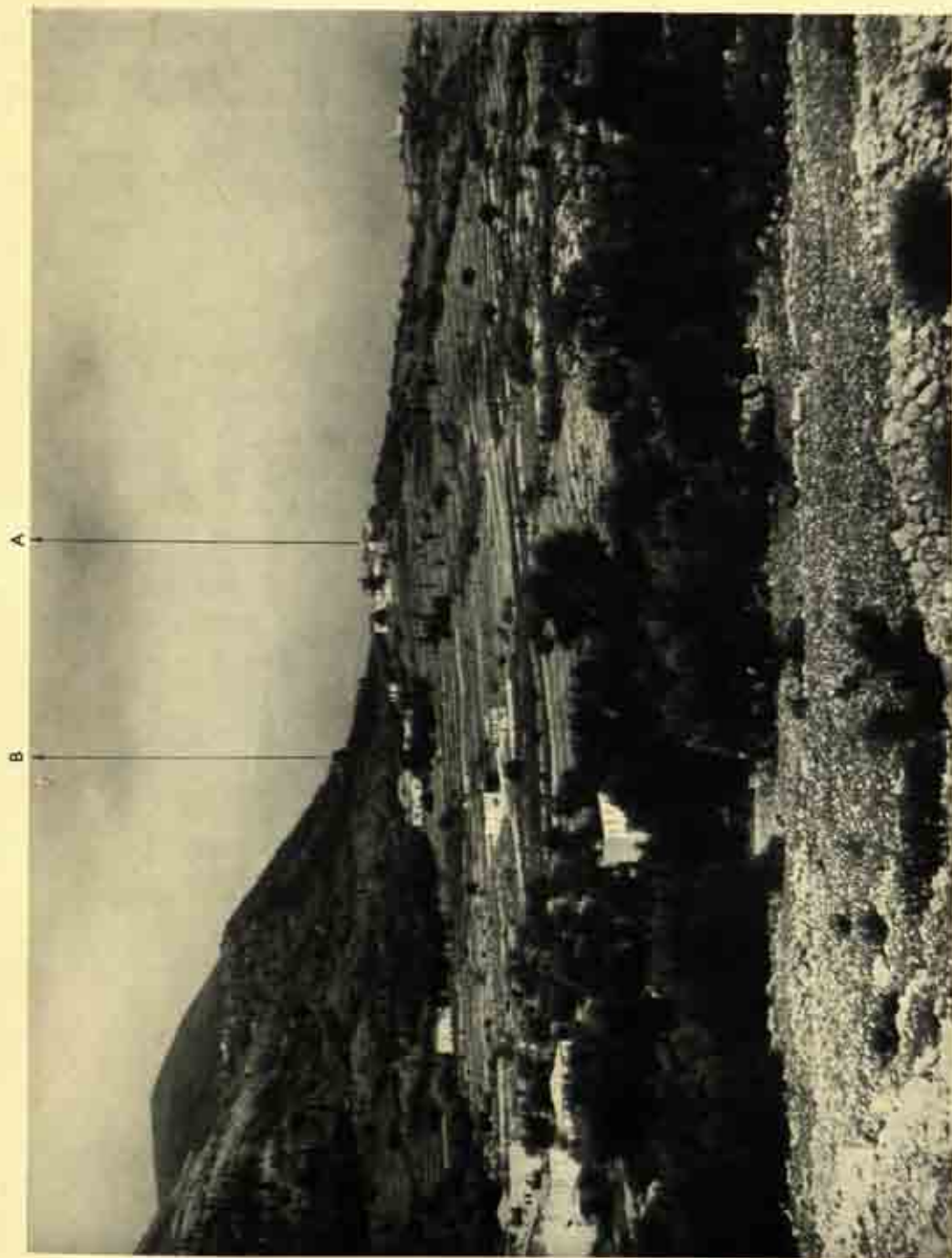


FIG. 5. PELICÁTA, FROM SOUTHWEST. (A) SUMMIT (B) HAGIOS ATHANASIOS

PLATE II

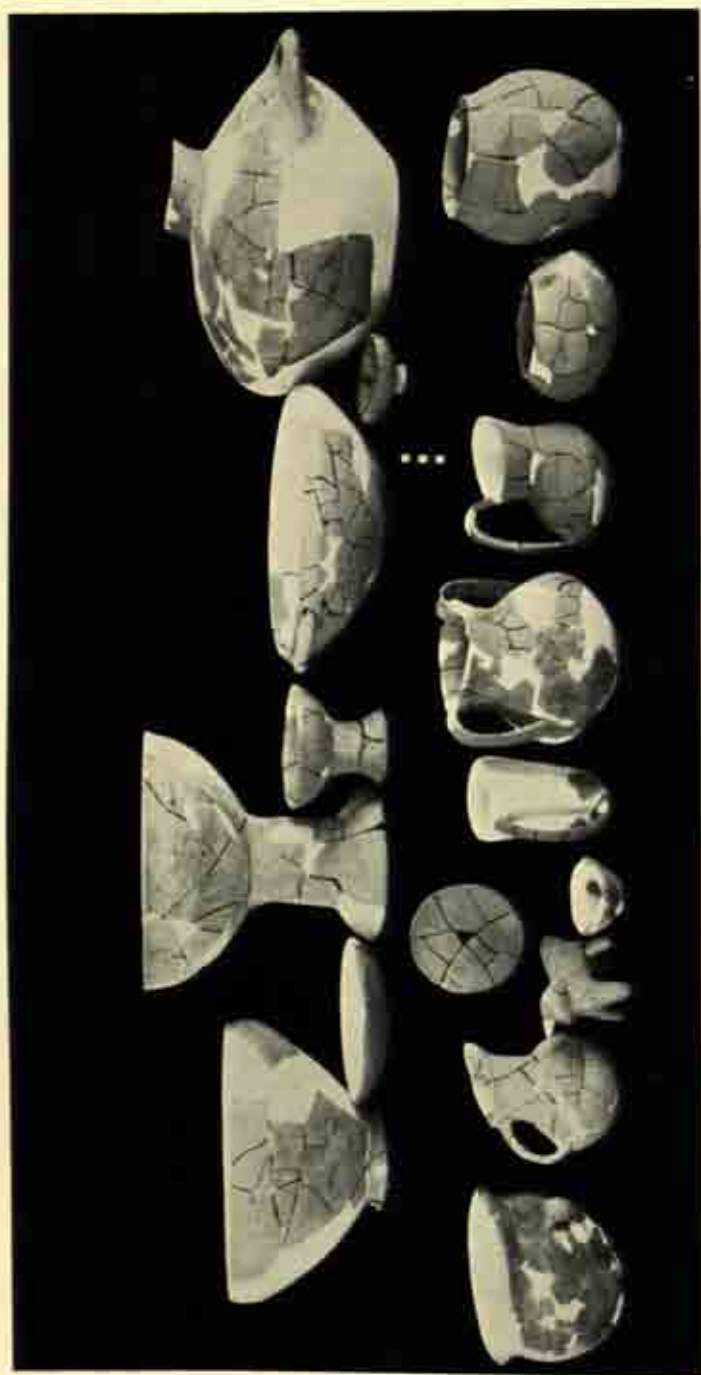


FIG. 6. EARLY HELLAIC POTTERY



PLATE III

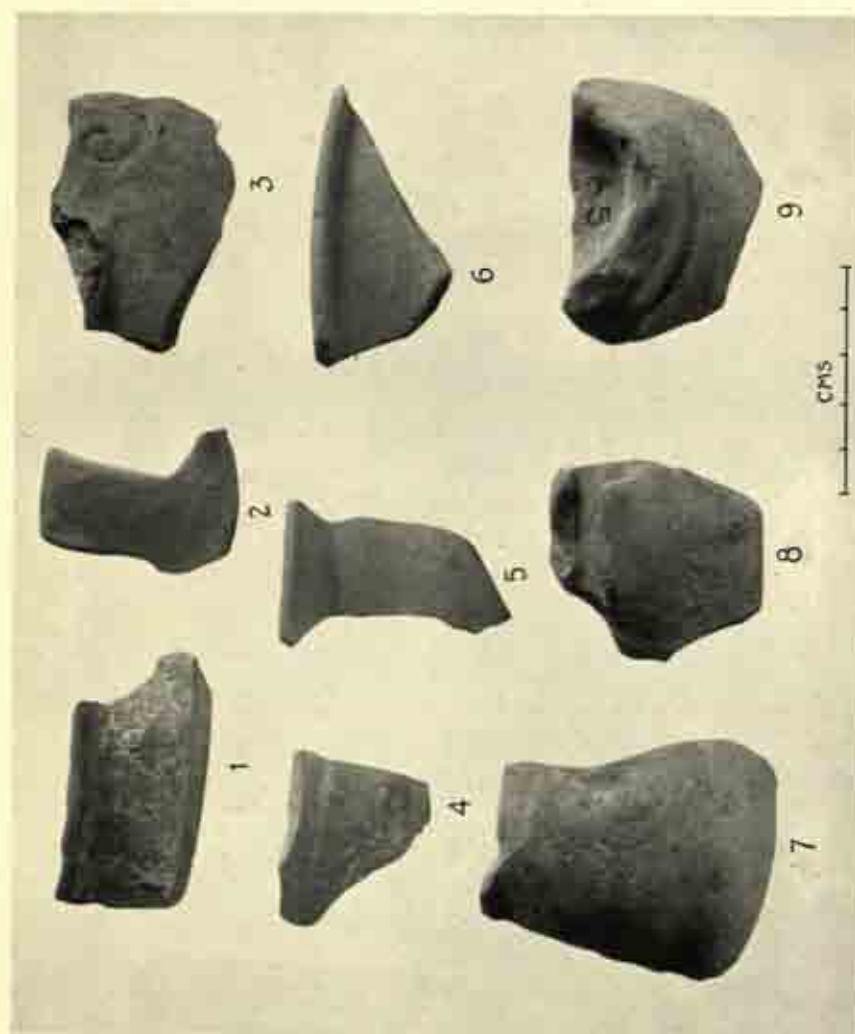


FIG. 2. "MINYAN" (MIDDLE-BELLARIC) POTTERY

PLATE IV

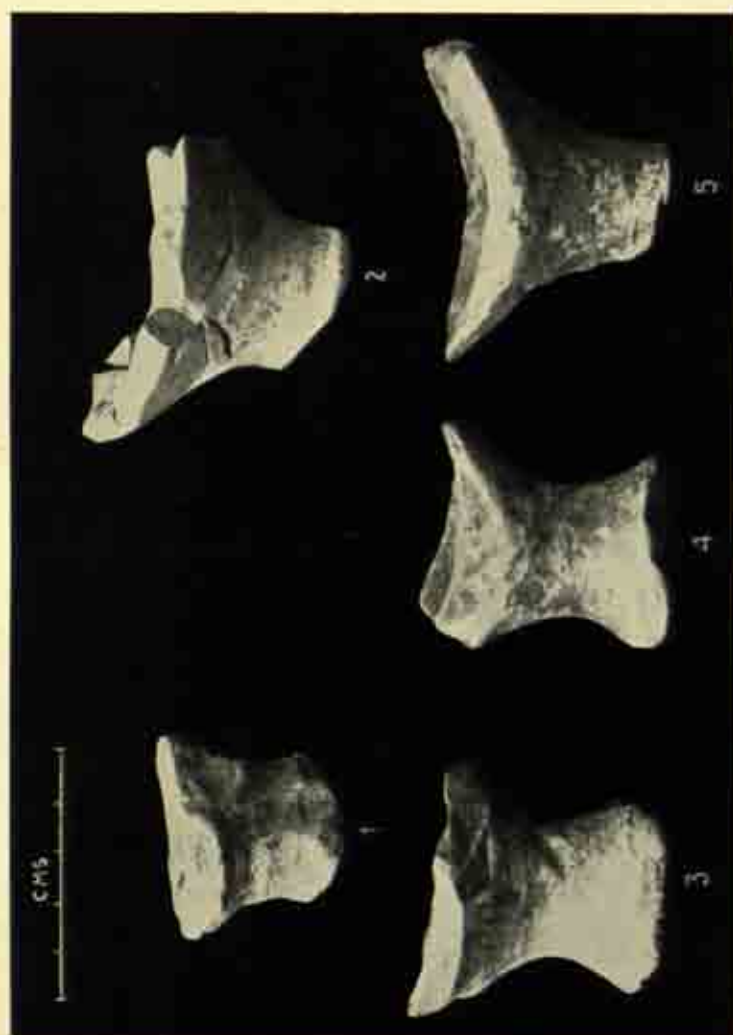


FIG. 3. MYCENAEAN FRAGMENTS



PLATE V



FIG. 9. ROCK-CUT TOMB



FIG. 10. DRESSED BLOCKS, POSSIBLY OF MYCENAEAN DATE

PLATE VI



FIG. 11. REMAINS OF ANCIENT CIRCUIT WALL.



PLATE VII



FIG. 12. LOWEST COURSE, REMAINS OF ANCIENT CIRCUIT WALL, PROBABLY *IN SITU*; UPPER PART MODERN





## THE SITE OF THE PALACE OF ODYSSEUS

place was associated with Odysseus several centuries after his death, it is clear that those who hold, on other grounds, that the hill of Pelicáta was in Homer's mind, when he described the palace of Odysseus, the city and its fountain, may now support their view by a certain amount of archaeological evidence.

The excavation of Pelicáta was part of the archaeological exploration of Ithaca, conducted by me on behalf of the British School at Athens, whose Committee I wish to thank for permission to use the photographs and plan published here.

## The Magic of Saint Oswald

by WILFRID BONSER

IN his article on the 'magic' of Columba (*ANTIQUITY*, June 1934), Mr Crawford uses this pagan word to signify the supernatural power which was ascribed to a Christian saint with regard to certain supernatural acts. In my title I am using the word 'magic' in the same sense, but I am limiting the scope of this article to the use of this magic by the Church after the saint himself was dead.

King Oswald of Northumbria was the first English martyr—Saint Alban being the first British one. Perhaps more miraculous cures are associated with him than with any other Anglo-Saxon saint.<sup>1</sup>

Cures were attributed to the cross which he set up on the Heaven-field before his victory there over the heathen Cadwalla. Bede says that

in that place of prayer very many miraculous cures are known to have been performed as a token and memorial of the king's faith; for even to this day [*i.e.* a century later] many are wont to cut off small chips from the wood of the holy cross, which being put into water, men or cattle drinking thereof, or sprinkled with that water, are immediately restored to health.<sup>2</sup>

Particulars of one more miracle through this medium are given. A monk of Hexham named Bothelm, who was still alive when Bede was writing, fell one night on the ice and broke his arm. A fellow monk was visiting Oswald's cross soon after, and brought him some of the old moss which grew on the surface of the wood. This the sick man put into his bosom, and awaking in the middle of the night, 'he found his arm and hand as sound as if he had never felt any such pain'.

<sup>1</sup> Details given by Bede in his *Ecclesiastical History*, book III, chapters 1-13.

<sup>2</sup> A similar story is told by Bede (*E.H.* III, 17) in connexion with Aidan. He died leaning against the post used to strengthen the wall of a church. Later the church was burned by Penda, but the post 'could not be consumed by the fire which consumed all about it'. The church was 'rebuilt in the same place, and that very post was set up on the outside, as it had been before, to strengthen the wall . . . And it is manifest that since then many have been healed in that same place, as also that chips being cut off from that post, and put into water, have healed many from their distempers'.



## THE MAGIC OF SAINT OSWALD

King Oswald fell in battle at Maserfield in 642, fighting against the heathen Penda of Mercia.

Wherefore many took up the very dust of the place where his body fell, and putting it into water, did much good with it to their friends who were sick. This custom came so much into use that, the earth being carried away by degrees, there remained a hole as deep as the height of a man.<sup>2</sup> (Bede).

It is not said if the earth at the bottom of the hole was less efficacious for healing than that at the top: the faith was evidently strong enough to surmount such a possible deficiency.

Some thirty years later Oswald's niece, Osthrida, queen of the Mercians, removed his bones to her monastery of Beardeneu [Bardney] in Lindsey. The monks would not at first give admittance to the relics, as being those of a foreigner who had subjected them to his rule and was therefore distasteful to them. The bones lay in a wagon outside the gates all night; but 'a pillar of light' reaching from the wagon up to heaven, was seen by almost all the inhabitants of the province of Lindsey'. The monks then received the bones, and having washed them, placed them in a shrine. The virtue in them at once became apparent. The monks 'poured out the water in which they had washed the bones in a corner of the sacred place. From that time the very earth which received that holy water had the virtue of expelling devils from the bodies of persons possessed'.

Bede gives details of two cures at Bardney. The first is that of a man 'who was on a sudden seized by the Devil, and began to gnash his teeth and foam at the mouth'. None could restrain him, nor were 'the prayers that were prescribed against that disease'—*i.e.* the usual form of exorcism used by the Church—of avail, but as soon as a casket containing the above mentioned earth was brought into the room, the patient fetched a deep sigh and said that all the evil spirits that vexed him had departed, and were no more to be seen. He was given a little of the earth to keep, presumably in case of a recurrence. The other cure was that of a little boy at the monastery who was troubled with an ague. One of the monks instructed him, 'Go into the church, and get close to S. Oswald's tomb; stay there quietly and do not leave it until the time that your fit is to go off; then I will go in and fetch you away. The boy did as he was advised, and the disease durst not affect him as he sat by the saint's tomb, but fled so absolutely that he felt it no

<sup>2</sup> Similarly with the earth where Aldhelm died: see Bede, E. H. v, 18.

<sup>4</sup> This is the usual method of revelation of relics and holy things.

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more'. The ague, as is usually the case, appears to have been regarded as a person.

After his death, Oswald was dismembered by Penda, and it is interesting to follow his various members in their travels. Penda caused the head and arms and hands to be set upon stakes, but Oswy took them down next year. He buried the head at Lindisfarne, and the arms and hands at Bamborough, where they were wrapped in a pall and enclosed in a costly shrine.<sup>5</sup> In 875 the monks of Lindisfarne fled before an incursion of the Danes, and carried the head, together with the body of Cuthbert, with them. It accompanied them in their wanderings from place to place until it finally rested at Durham. It was found in Cuthbert's tomb when this was opened in 1105, and the frontal bone of the king was again found and replaced when the coffin was examined in March 1899.<sup>6</sup>

But the majority of the miracles were connected with the right arm and hand. Aidan had once sat next to the king when he was distributing food to the poor, and, struck with his piety, had 'laid hold of his right hand and said, "May this hand never perish". Which fell out according to his prayer', continues Bede, 'for his arm and hand, being cut off from his body when he was slain in battle, remained entire and uncorrupted to this day, and are kept in a silver case, as revered relics, in S. Peter's church in the royal city' [Bamborough]. They were stolen from Bamborough by a monk of Peterborough named Vynegot in the time of Abbot Elsin (died 1055), who was a most diligent collector of relics. Peterborough's 'most precious relic', says Hugo Candidus, the abbey's chronicler who wrote in Henry II's reign,

is the right arm and hand of S. Oswald, remaining whole in flesh and skin after the blessing of S. Aydan, the bishop: this we have seen with our own eyes and have kissed and handled and washed when it was shown to Alexander, bishop of Lincoln [and others] . . . 487 years after the martyr was killed . . . They also possess one of his ribs and some of the ground on which he was killed.<sup>7</sup>

When Peterborough was burned by the Danes and Hereward the Wake in 1070, prior Athelwold saved the arm and hand by secreting

<sup>5</sup> Simeon of Durham, 'History of the kings of England', under date 774, and again in his history of the church of Durham, chap. 2.

<sup>6</sup> See J. C. Wall, *Shrines of British Saints* (Antiquary's Books, 1905), pp. 206-7. The statue of Cuthbert in Henry VII's chapel at Westminster represents him as carrying the crowned head of Oswald in his left hand.

<sup>7</sup> Hugo Candidus, p. 34, in 'Historiae Coenobii Burgensis scriptores varii' (ed. Sparke, 1723).



## THE MAGIC OF SAINT OSWALD

them in the straw of his bed.<sup>8</sup> The monastery was again burned down in 1116, and the new church was not opened for worship until 1140. At the opening ceremony the bishop of Lincoln and the abbots of Ramsey and Thorney were present. 'The arm of S. Oswald was on show', says Hugo,

but first [it] was shown to Abbot Martin who desired to see it either out of curiosity, or because he doubted if it were whole . . . They opened the case and brought it out whole and with it blessed the whole congregation, and washed it in great fear as they had done before in the time of Abbot Matthias. Many of the monks who were sick men when they entered the church were [completely] cured. And the third time that it was shown was to King Stephen when he came thither and offered to it his ring.

'Through the merits of S. Oswald', continues Hugo, 'and through the water in which the arm is washed, many sick persons are healed and are liberated from demons and *paralytici* and *frigoritici* are cured'. Two examples of such cures follow. 'The water was carried through many districts and drunk and many were cured according to the faith of each one. It was taken to London, and we know that innumerable persons were cured there, wherefore his altar in the crypt of S. Paul's was held in great veneration'.<sup>9</sup>

The left arm seems to have been preserved at Gloucester. Like other relics, however, king Oswald's arms showed a tendency to multiply, for another copy of the left arm appears to have existed at Durham, and an arm (which of them not being specified) is mentioned in an inventory of 1245 of S. Paul's Cathedral, London.<sup>10</sup>

The body of the king was probably buried first at Oswestry, but was taken thence to the abbey of Bardney in Lindsey, as already mentioned. The Anglo-Saxon Chronicle says that his bones were removed thence by Ethelflaed, Lady of the Mercians, to a monastery built in his honour at Gloucester. This was owing to a war of Edward the Elder with the Danes in Northumbria.

Many of Oswald's relics were also transferred to the continent. The cathedral of Zug, for instance, received some on its dedication to him in 1478; that of Hildesheim still possesses in its treasury a 13th century casket which is said to contain a head of Oswald, the crown of

<sup>8</sup> Hugo Candidus, p. 50.

<sup>9</sup> *Ibid.*, pp. 77-8.

<sup>10</sup> 'Brachium Sancti Oswaldi coopertum foliis argentiis, praeter manum'.—W. Sparrow Simpson, 'Two inventories of the cathedral church of St. Paul, London'. (*Archaeologia*, 1887, vol. 50, p. 470).

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which is of 11th century workmanship. Bede speaks of his renown spreading to Germany and Ireland, and gives details of a cure in the latter country. A 'certain scholar of the Scottish race' being about to die—presumably through fear that he should be carried away to hell for his sins—asked Acca if he had in his custody any relics of Oswald, since he thought it was possible that through them his life might be prolonged. Acca replied, 'I have some of the stake on which his head was set up by the pagans, and, if you believe with a sincere heart, then the Divine Goodness may, through the merit of so great a man, both grant you a longer term of life here and render you worthy of admittance into eternal life'. When the patient had assured him of his faith therein, Acca blessed some water, put into it a chip of the stake, and gave to the sick man to drink. Needless to say he recovered and, changing his mode of life, lived for a long time after.<sup>11</sup>

It is interesting to note from the stories in connexion with these relics, the means employed for extracting 'virtue' from them, so that it might be applied for healing purposes.

The four sources of virtue in connexion with Oswald were (a) the cross which the king set up, (b) the stake on which his head was impaled, (c) the ground on which he fell, and (d) the various parts of his body. The chips of the cross and of the stake were immersed in the water whereof the patient drank: the water in which the uncorrupted arm had been washed was also given by the Peterborough monks to the sick to drink: but the mere presence of the earth or dust appears to have been sufficient to give the necessary faith to the sufferer. The dust and water cures seem to have been combined with regard to the bones, for apparently after the water in which these were washed had been poured on the ground, this earth when dried to dust was considered to retain the healing properties derived from the water. This may have been used as a plaster, possibly its mere presence again sufficed.

If a cure was not effected at one shrine, a patient might be carried to others in turn. Each monkish chronicler was naturally keen for the reputation of the relics at his own monastery, and uncritical of the medical aspect.

It is obvious that the faith—or credulity—of the ordinary patient was such that, once the healing reputation of a relic had been established, its possessors by their ingenuity were able to work the oracle in various ways. When, instead of doing good merely for the glory of their

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<sup>11</sup> Bede, E. H. III, 13.



## THE MAGIC OF SAINT OSWALD

religion, the element of greed entered in, and when payment was demanded in return for the cure, the spirit of the early Anglo-Saxon Church entirely disappeared. The atmosphere of deceit already prevailing on the continent only entered in when the religious life in this country began to decay. This is well illustrated by the appearance of the duplicate copies of Oswald's arms already mentioned.

There is no reason, however, to suspect that the various Anglo-Saxon relics which were preserved in the earlier period were other than the genuine articles. The purity of the early Church in England was too great and the sincerity of the higher clergy is too obvious from the accounts preserved by Bede and others. The Anglo-Saxon saints were men and women of great sanctity in the eyes of their contemporaries who, realizing this, conserved their relics as the greatest treasures they possessed. They also believed in these early days in the efficacy of the relics for healing purposes. Being the most accredited physicians of their time, they nobly did their duty in effecting 'faith-cures' by these means wherever possible.

## Recent Books on British Archaeology

by W. F. GRIMES

THE problem set to the reviewer who seeks within a few short pages to comment upon a number of books as varied as the present, is not an easy one. Detailed criticism or commendation is obviously impossible; but since in their different ways these books\* illustrate very well the range of contemporary archaeological work in this country, I have selected certain features to which they bear some relation, and which seem to be of sufficient interest and importance to warrant discussion.

One of the most striking features which must present itself to the reader of Kendrick and Hawkes or of the Congress Publications is the way in which new methods have changed the face of archaeology in Britain.

Mr O. G. S. Crawford has dealt elsewhere<sup>1</sup> with the development of the study of antiquity, and has shown how what may be called a collector phase dominated the situation for many years.

The preoccupation with the specimen for its own sake which resulted from this phase culminated in the production of elaborate typological sequences, the outcome of which was to establish a chronological system with clear-cut divisions, each having its own group of characteristic forms of implements and so on.

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\* Archaeology in England and Wales, 1914-1931. By T. D. Kendrick and C. F. C. Hawkes. Methuen, 1932. pp. xx, 371, 30 plates, 123 text-figures. 18s.

Proceedings of the First International Congress of Prehistoric and Protohistoric Sciences, London, August 1932. Oxford University Press, 1934. pp. vi, 322. 21s.

A Handbook of the Prehistoric Archaeology of Britain. Oxford University Press, 1932. pp. viii, 75, 22 text-figures. Price not stated.

An Introduction to the Archaeology of Wiltshire (2nd edn., revised and enlarged). By Mrs M. E. Cunnington. Devizes: C. H. Woodward, 1934. pp. xii, 167, 43 text figures. 4s.

Celtic Ornament in the British Isles down to A.D. 700. By E. T. Leeds. Oxford University Press, 1933. pp. xx, 170. 12s 6d.

Congress of Archaeological Societies: report of the 40th and 41st congresses and of the Research Committee for 1932, 1933. London, 1935. pp. 60. 1s.

<sup>1</sup> The Sociological Review, April-June, 1932.



## RECENT BOOKS ON BRITISH ARCHAEOLOGY

While no one would seek to decry the value of these sequences, since they may sometimes provide a chronological basis upon which to build, clearer insight into the activities of early man has been obtained by studying, not so much the internal evidence of the specimen, as its cultural significance and external relations to its context. Purely typological studies, by failing to take account of these other factors, may entirely obscure the cultural changes which are the real stuff of which civilization is made.

Divisions are therefore being based more and more upon cultural phases, instead of upon a purely chronological scheme whether relative (in terms of sequences) or absolute—the latter in any case being still largely a matter of uncertainty at the present time; and such an arrangement meets much more satisfactorily the special needs of a country like Britain, with its unique position in relation to the continental sources of culture, and its own internal arrangements.

In the study of all phases from the dawn of the human period downwards these changes appear. In the Bronze Age, for instance, the chronology proposed by Montelius, with its five phases based upon a purely typological classification, has now been replaced by a simpler arrangement of Early, Middle and Late, which has a more direct bearing upon the course of events as they are now known. The Early phase thus becomes the period of Beaker dominance, the Middle that of consolidation and development of the fused elements of beaker and pre-beaker stock (the latter predominating) into a 'native' Bronze Age culture; while the Late Bronze Age witnesses a fresh series of invasions bringing the continental Late Bronze culture. The result is to re-create the Bronze Age as a vital phase in human prehistory: things are given their correct relation to the men who made and used them, and one is spared the uncomfortable and bewildering sense of delving into a catalogue of prehistoric hardware.

In the Iron Age also the continental divisions of Hallstatt and La Tène have now been generally replaced, as far as this country is concerned, by a three-fold cultural division of Iron A, B, and C<sup>2</sup>, even when, for purposes of general chronology, the sub-periods of the older system are retained.

Mr Leeds's monograph on the art which was perhaps the most outstanding product of the Age is of some interest for the question of

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<sup>2</sup> Since the above was written Dr Wheeler has suggested further modifications of this classification. *Antiquaries Journal*, 1935, pp. 273 ff.



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method with which we are at the moment concerned. Here it may be suggested that Mr Leeds's treatment commands the greatest degree of acceptance in the present state of knowledge, where it relies upon external evidence such as association and distribution in conjunction with the internal evidence of typology and style. Thus his recognition of various schools of Celtic art, closely linked (as in the case of the western school with the southwestern part of the Iron Age B movement) with one or other of the cultural phases must appeal to most people who are familiar with the material. There can be little doubt that these divisions will stand the test of time, at least in broad outline. When, however, reliance is placed upon internal evidence to the exclusion of other factors such as association, the more cautious amongst us may perhaps be forgiven if we hesitate—even while acknowledging the force of Mr Leeds's arguments—to carry subjectivity so far.

Recent years have seen also a growing realization on the part of archaeologists of the value of the results which may be obtained from the cooperation of the more securely-established natural sciences. One of the oldest examples of this is of course Zoology, which by identifying the animal-remains in the deposits has enabled the climatic conditions of the various phases of the palaeolithic to be established. More recently the methods of Botany in the pollen analysis of peat deposits,<sup>3</sup> and of Geology in the identification of the source of the 'blue-stones' of Stonehenge, may be mentioned as extensions of the same idea.

At the present time only the fringe of this aspect of our work has been touched, but it is at least possible that mutual advantage will result from it. The solution of such geological problems as the dating of the comparatively late land-movements, for which there is evidence in submerged land-surfaces and other remains about our present coasts, may well be assisted by the presence of human artifacts which the archaeologist may be able to place with some precision. This, indeed, has already to a certain extent been done, but a greater awareness of the problems, and the development of methods of co-ordination, should enable us to make fuller use of the opportunities which will present themselves in the future. So, too, the zoologist<sup>4</sup> or the botanist may obtain useful information bearing upon the history of animals or plants from the presence of their remains in archaeologically-dated deposits. We may therefore reasonably suppose that all branches of study will benefit from the association. At the same time, it is well for

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<sup>3</sup> *Congress Proceedings*, p. 105.

<sup>4</sup> *ibid.*, pp. 151 ff.



## RECENT BOOKS ON BRITISH ARCHAEOLOGY

archaeologists to remember the obligations which the methods and materials of the older sciences must legitimately impose on them.

It goes without saying that non-archaeological material of botanical, geological or other interest will be preserved with the same care as purely archaeological evidence by any competent excavator whose aim is to extract the fullest information from his site. The importance of such material for the question of environment—quite apart from its potential value from the purely botanical or geological point of view—must be self-evident; but its full value can only be realized when every precaution is taken to preserve structure and other features (as, for instance, ancient timbers) as unimpaired as possible.

There are also, however, certain types of archaeological material for the fullest use of which the methods of the other sciences have to be employed. And here various considerations suggest that we are not yet entirely free from the view (a relic of the collector-phase to which we have already referred) of our specimens as almost sacrosanct and an end in themselves, to be preserved unsullied at all costs. This is particularly the case if the specimens happen to be 'good' ones; but if all classes of archaeological material are to be utilized to the utmost, to extract from them the greatest amount of information relating to man and his works, it is clear that this view must go.

One of the most interesting and valuable developments of collaboration between archaeology and geology has been in the determination of the sources of rock materials. Obviously the best example of this is the now famous identification, due to the late H. H. Thomas, of the source of the 'blue-stones' of Stonehenge in the Presely mountains of southwest Wales. The method has, however, already been applied, though on a limited scale, to the materials of which 'green-stone' polished or ground axes were made, and already important facts are beginning to emerge.

There is, for instance, the presence in Wiltshire of axes of igneous rocks which are at home in North Wales;<sup>1</sup> while conversely flint axes have been found in Wales, where flint of the size and quality necessary for such implements apparently does not naturally occur. The hypothesis of a natural means of transport for the raw materials can be eliminated, and these facts therefore become important because of their bearing upon contemporary movement, whether of trade or settlement.

A moment's thought of the large number of greenstone and other

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<sup>1</sup> *Congress Proceedings*, pp. 137-8.



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axes to be found in public and private collections all over the country will be sufficient to give some idea of the vast amount of potential evidence waiting to be tapped by the worker competent to examine it. But the problem is essentially one for the petrologist, and when, as in due course it must, the work is tackled on scientific lines, the only satisfactory method of approach can be a geological one.

Up to the present the bulk of the identifications that have been made have been based upon macroscopic examination of the surface of the implement, but there can be little hope of any real advance until a more certain method is more generally adopted. Exposure to a variety of influences brings about weathering of the surface of the stone, and produces features which may be quite different from those of the unchanged rock at the core of the specimen. To expect accurate determinations from such evidence is to place an unfair burden on the geologist on whom the archaeologist must depend; but in any case the value of the result thus obtained must clearly be open to very serious question: it is generally uncertain, and may very frequently be wrong. The only sound basis is the provision of a fresh surface of the stone, unaffected by weathering and other factors; and this can only be obtained by the deliberate cutting of the specimen. The result would be to replace guesswork by certainty (obviously the only possible basis for a subject which professes scientific principles); and the necessary vandalism could in point of fact be perpetrated with comparatively little damage to the object itself. The principle by which destruction of some part of the evidence is recognized as inevitable by the older sciences can readily be adapted to meet the somewhat specialized need of archaeology, in which every specimen is in a sense unique and irreplaceable.

I make no claim to originality of any sort in setting forth the above views. Indeed, I believe them to be widely held, at least amongst the most advanced workers of today, by some of whom they have been put into practice. But while what may be (somewhat sweepingly) called the 'collector mentality' seems still to be sufficiently strong among us to justify this re-statement, such a change need not be inconsistent with the reverential treatment which all museum specimens should receive, if it is carried out carefully and with understanding, as has been done with Mr Alexander Keiller's collection of greenstone axes. The evidence thus obtained will be of value; the alternative, evidence based on false or at least uncertain premises, becomes on the face of it pure retrogression.



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If I have appeared to stress unduly the problem of the relation of archaeology to the established sciences, and of our technique to the principles upon which they have been so successfully built, it is because I believe that only along these lines can permanent advance be made. The longer such changes as are necessary are postponed, the greater will be the loss to science ; nor can we claim full stature for our subject as a science until (no doubt among other things) our technique in the collecting and examination of evidence has been placed upon a sound objective basis.

We cannot, however, leave this aspect of the subject without considering another matter to which attention is drawn by another of the books with which this article is concerned.

Concentration upon distributional work in the past few years has shown that close relationship existed between the movements and settlement of man on the one hand and the various factors that go to make up environment on the other. The working out of successive distributions has emphasized this relationship, and has been sufficient to show that on general grounds the argument holds good : the history of human affairs since the Pleistocene period has been one of gradual expansion from areas of primary settlement (which were naturally open regions) to other less-favoured parts which could only be settled as civilization achieved the necessary organization and means for the clearance of natural obstacles such as forest and swamp.\*

But the brilliance with which the material has been handled must not be allowed to blind us to the dangers which attend too facile a use of these arguments. In many areas variations present themselves, making it dangerous, if not impossible, to apply the general thesis except in modified form. And while more can still be learned from study of these problems in their widest application, it is none the less true that the time is coming when we shall need more detailed local studies, in order to examine more fully these variations, and to see to what extent modification of the general position is desirable.

In saying this, I am, of course, aware that a number of local surveys already exist. The County Archaeologies published by Messrs. Methuen are very helpful in a number of ways as far as they go ; so too the volumes of the Victoria County History. Mrs Cunningham's *Archaeology of Wiltshire* is a local study which provides within its covers a broad picture of the pre- and early history of one area set

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\* *Congress Handbook*, pp. 17-19.



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against the background of the archaeology of southern Britain as a whole. Such a book, written as it is by the acknowledged authority, has a definite value, even for the expert, but its purpose nevertheless is essentially popular: the background is lightly and conservatively sketched; the descriptive matter follows a somewhat unusual scheme which is in essence topographical and drawn on the broadest lines. It is no disparagement of this and similar studies to say that they do not meet the rather specialized need which soon must make itself felt.

Here again I make no claim to any particular originality when I suggest that what seems to be called for is a series of field-surveys, not necessarily by counties, having as their ultimate aim the preparation of distribution-maps on which the remains of early man may be studied against the reconstructed natural environment of a given area. Again, too, I am aware that maps on these lines have already been prepared for certain areas. But the point which I should like to urge, and which I feel cannot be too strongly urged at the present time, has to do with the attack upon the problem of environment. In the preparation of the base map it will not be sufficient to accept generalizations, nor even to build up a reconstruction of the supposed natural conditions upon the basis of a geological map, whatever its scale. It will be necessary to study on the ground variations in soil-character, vegetation, and other factors bearing on environment; in other words we must re-affirm the principle that we can only obtain and use geological (including under that term for convenience the various aspects of soil, drainage and so on) and botanical evidence by following geological and botanical methods. The problem of the application of those methods is one which cannot be gone into here. Obviously the work could best be carried out by the pedologist and botanist; just as obviously the archaeologist will need their expert cooperation and advice if he sets out to do it himself. The special nature of the task may indeed demand a special technique in which the necessary elements will be borrowed and adapted—and we may perhaps hazard the prophecy that the time will come when a rather specialized grounding in the natural sciences will be regarded as an essential part of the training of the field archaeologist. There will perhaps be some to argue that in adopting such a course archaeology goes unnecessarily beyond its sphere: for them withdrawal from a field which promises valuable results would seem to be the only logical step. But if man's relation to his environment is to be regarded as an integral part of archaeological work, as present tendencies increasingly suggest, then the time is ripe for setting this



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side of the subject upon a similar footing to that upon which our technique of excavation has been established with such satisfactory results. The essential facts are that we can no longer work on anything but scientifically-established data, and that argument from the general to the particular, or even from one particular to another, is an even more dangerous process than usual when the subject is the wide variety of natural conditions of which these islands are composed.

Only less important than the question of the cooperation of archaeology and the national sciences is the question of future organization. Growing realization of the bulk and complexity of the work now being produced over a wide field has awakened the need for close cooperation between workers. The result has been the appearance in recent years of a series of summaries of new discoveries and current work, which have now become a feature of several periodicals. Students as a whole are thus kept in touch with the latest events, frequently before publication in any detail is possible. This type of organization, however, may well be left to take care of itself; the close contacts that nowadays exist between individual students and societies are sufficient guarantee that ideas and discoveries will circulate freely, even if the necessary public-spirited workers are not available to put them into print.

The second type of organization sets out to attack a problem which can only be solved by the cooperation of a number of workers united under a closely organized scheme.

Obviously the best instance of this—the best if only because it is furthest away in time, and its results can therefore be the more accurately computed—is the Earthworks Committee which was set up by the Congress of Archaeological Societies in 1901. This Committee published in due course a scheme for the direction of workers on earthworks, with a series of types, recommendations as to scales, features to be noted, and so on. With the details of that scheme we are not concerned here. The main fact to be noted is that while the Committee produced no official survey of the type foreshadowed by its proposals, it nevertheless provided the basis for a great deal of good work. Directly or indirectly, such pioneer workers as Hadrian Allcroft, Williams-Freeman and Heywood Sumner derived inspiration from it, and its classification of sites was adopted by the Victoria County History. The result has been a large body of material of very great value to the student of earthwork, which could hardly have been assembled by any single individual. The fact that the material does not—as yet—cover the whole country is obviously beside the point: in this incidental events



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have played their part. The work still goes on, and the influence of the Earthworks Committee is still strong.

More recently two other schemes have been advanced for a similar purpose, both under the control of a Committee set up by the British Association.

The first of these was established in 1913 as a Committee 'to report on the Distribution of Bronze Implements',<sup>7</sup> and rightly thought its first duty to be the preparation of an index of all the known implements which should form the basis for further work. The British Association card catalogue, which is well known to all students of prehistory, was the result of this Committee's labours, with the aid of voluntary workers under the general direction of Mr Harold Peake. Now regarded as complete for this country, the catalogue has been placed in the care of the Department of British and Medieval Antiquities of the British Museum, and the hope is expressed that future discoveries will be brought to the notice of the Museum authorities, so that the specimens may be drawn and incorporated in the catalogue. To this arrangement I refer again below.

The second Committee was established in 1919 'to report on the Classification and Distribution of Rude Stone Monuments'.<sup>8</sup> Here also the first step was to be the preparation of a classified index of megaliths, the field work being carried out under the direction of the central Committee. Here unfortunately the results have not been so successful, and the difficulties of coordination and direction which appear in a minor degree to have beset the Bronze Implements Committee in its work seem to have operated on an even more intensive scale in the case of the Megaliths Committee.

This leads me to suggest that archaeology in this country has reached the stage when the collecting of evidence sufficiently detailed to be of value on such important and complex subjects as megaliths has grown beyond the scope of voluntary committees with part-time officers, however distinguished and scholarly those committees and their officers may be. That the actual collecting of the evidence should fall mainly to the lot of local workers—where they exist with the necessary qualifications—is natural, and, indeed, inevitable, if completeness in the record is thought to be desirable; but the work of organization and direction, the coordination of the material when collected, should be the work of one or more full-time experts with

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<sup>7</sup> See the *Congress Proceedings*, p. 145.

<sup>8</sup> *ibid.*, p. 119.



## RECENT BOOKS ON BRITISH ARCHAEOLOGY

adequate clerical assistance. At the present time no organization capable of dealing with such a situation exists.

Here we may return for a moment to the Bronze Implements catalogue, which, we have already noted, has been handed over to the British Museum, accompanied by a pious hope that future discoveries will be reported and added as necessary.

One of the first essentials of such a catalogue is that it should, within reasonable limits, be kept up to date. The greater the lapse of time between additions, the harder the task will become, and the greater will be the decline in value of the catalogue. Once again it appears that no adequate permanent machinery exists for such a purpose. No one, least of all the present writer, will question the willingness of the British Museum officials at all times to place their resources at the disposal of the research-worker. On grounds of general accessibility no difficulty would arise. But the mere fact that we have become accustomed to treating the British Museum as the unofficial centre of archaeological studies can be no excuse for perpetuating an arrangement which, whatever the general benefits, places an undue burden upon a few individuals. We have no right to expect that the Museum staff will be able to undertake the work of maintaining the catalogue as it ought to be maintained, while the curators of provincial museums and others upon whom is to devolve the task of supplying the information are already for the most part fully occupied in their routine duties of preservation and education. It is too much to hope, therefore (in the absence of a central driving force) that the record will keep pace with discoveries. In this mood of pessimism I can only conclude that the catalogue must in course of time lose all practical value as the complete record which it is intended to be, unless means can be found of establishing an efficient liaison-system based upon something more reliable than the present arrangement.

The obvious conclusion is that our subject needs, or must soon need, some kind of central organization to act (among other things) as a repository of records of the raw materials which students use. The growing complexity of archaeological work, and the difficulties attendant upon it are carrying us beyond the days of voluntary, and frequently unwieldy, committees as our only means of attacking problems of any size for which whole-time experts are necessary. The gap can best be filled by the Institute of Archaeology which has recently been formed, and to re-state the need for that Institute here is merely to repeat what has already been said with greater authority by others.

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The publication of material assembled in this way is an uncertain matter which depends upon so many factors ; but the fact that the material would be available in one form or another in one centre would in itself be of outstanding importance. A moment's thought will enable one to visualize some of the possibilities. Mr Leeds's monograph suggests one. A fully documented corpus of all the known examples of Celtic art, whether published or merely in the form of an index, would be of untold value to the student, whether of human history or of art-forms. A central Institute (working, it may be, in association with a Committee of the acknowledged authorities, but still exercising unfettered control) would seem to provide the only machinery for carrying out such work profitably and in good time. This, I hasten to add, must not be understood to imply that the work is necessarily to be carried out by members of the Institute themselves. The dangers of excessive centralization are almost too well recognized in every branch of life in this country ; but no less for the sake of the museum- rather than for the research-worker—though they are frequently one and the same—we must distinguish between the preserving and educational functions of the museum as such, and the training, research and directional functions of the Institute. The two must obviously maintain close cooperation (the British Museum will be no less indispensable to any research organization, because of its collections) : neither is complete without the other. Until the Institute is equipped to meet the needs outlined above, and established as a working force, a vital need of archaeology in this country awaits fulfilment.



## The Writing of Njoya

by O. G. S. CRAWFORD\*

EVER in the background of anthropology there lurk certain ghosts, doomed to haunt us until the doubts they stand for shall have been laid at rest. They hover around in couples, mutually opposed and irreconcilable. There is the ghost of Diffusion and his sprightly spouse, Independent Invention; there are the Race-and-Culture Twins, and their half-brothers Innate Capacity and Cultural Achievement. (There are also a few others whose time of rest is near at hand). So long as we keep on the move, we need not fear these ghosts; but if we stop, still more if we lie down and go to sleep, they attack us with darts in the form of query-marks, some of them, alas, dipped in the poison of acrimony, or barbed with political animosity.

This haunted background is illuminated by the remarkable achievements, during the first decades of this century, of Sultan Njoya, ruler of the Bamoun communities of the Cameroons. Perhaps his most brilliant feat was to invent an ideographic script for the Bamoun language; and then, not content with this, to convert it into an alphabetic one, and to use it for the promulgation of a code of laws and a religious or philosophical system. He died in 1932, but his alphabet is still in use.

Three other original systems of writing are known on the west coast of Africa and in the adjacent regions. (By 'original' is meant systems that consist of signs invented by the natives themselves and coordinated to represent the sounds of their language). The first was discovered in 1848 by Commodore Forbes of the British Navy; his results were described and published a few months later by a missionary, Koelle. Since then several writers have become interested in this

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\* The first part of this article, after the two opening paragraphs, is a free translation of the chapter on Bamoun Writing, by M. Henri Labouret, published in the journal *Togo-Cameroun* (11, rue Tronchet, Paris), April-July 1935, pp. 127-133. I wish here to acknowledge my indebtedness to M. Labouret for allowing me to publish this translation and for authorizing the form in which it appears; and to the Editor of *Togo-Cameroun* for supplying the material for the illustrations. The concluding remarks (p. 439 to the end) are my own.

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script, which is probably ancient and remains in use amongst the Vai or Vey, a people speaking a Mandingo dialect and living on Liberian territory, between the river Gallinhas and Monrovia.

Another system was reported in 1905 by District-Commissioner J. T. Maxwell amongst the Efik of Calabar province. Though it has been much studied, it is not possible to state definitely that it is a form of writing. Some regard it as simply a large collection of signs of different values used by the societies corresponding to age-classes of the young people.

On the other hand there can be no doubt that an original system was invented in the Cameroons by the personal initiative of Njoya, Sultan of Bamoun, in 1900. The existence of this writing was revealed to the world in 1907 by the German missionary Goehring, who published a very interesting note about it.<sup>1</sup> Since then several writers have studied this very remarkable creation. The most complete and the clearest analysis is that of Maurice Delafosse.<sup>2</sup> Nevertheless, as regards the history of the subject, several of his statements are contested by the present Sultan and members of his court.

To begin with it may be said that the occupation of Foumban by the Germans in 1899 could have had nothing to do with the Sultan's invention of signs that should be incomprehensible to Europeans as well as to the natives knowing Arabic writing. The Bamoun were then already Musulmans; many of them could read the Koran, and used Arabic in business correspondence with the merchants recently arrived from the north. But we must define our terms, when we speak of the German 'occupation'. The Germans could hardly have caused any inconvenience even to the inhabitants of Foumban, for their administrative station was situated at Bamenda, more than 50 miles distant, across a range of mountains. The Sultan was represented there by clerks who translated his reports to the Resident. It was not until 1910 that an agricultural station was created at Nkoutie; then in 1913 the district of Bamenda was transferred by Captain Adamest to Matunke near Nkoutie. In actual fact the Germans were never in the immediate neighbourhood of Njoya and did not attempt to molest the Sultan.

But the sovereign ruler of the Bamoun was a man gifted with great intelligence, backed by a lively imagination. He fully realized the importance that writing might have in the development of his people

<sup>1</sup> 'Der Koenig von Bamun und seine Schrift'. *Der evangelische Heidenbote* (Basel), vol. LXXX, no. 6.

<sup>2</sup> *Revue d'Ethnographie et des Traditions populaires*, 1922, pp. 11-36.



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and he strove to bring about what he regarded as an urgent improvement of their culture. Later, thinking that the local religion too needed reforming, he composed a most curious syllabus of philosophy, which was influenced by the doctrines of Islam and Christianity and united to them by animistic ideas.<sup>3</sup>

Over a long period he concentrated all his faculties on the task of creating the signs he wished for. One night he had a dream in which there was revealed to him a complicated figure corresponding to the










Bamoun word	Meaning	Sign of 1900 (Clapot)	Sign of 1907 (Goehring)	
<i>Pè</i>	<i>Kola-nut</i>			
<i>Fom</i>	<i>King</i>		1907	1916
				
<i>Ntab</i>	<i>Hut</i>			
<i>Nyad</i>	<i>Ox</i>			

FIG. 1. BAMOUN IDEOGRAPHIC SCRIPT

letter 'pouen': hand. He dreamt that he wrote this letter on a slate and that after washing the surface of the slate clean again, he drank the water. Thus he was performing a magic rite well known to the Muslims and one which he believed would assist his investigations. With the help of several court officials he drew up a preliminary list of signs to be employed. These, most fortunately collected by the missionary Goehring, and later by the French Lieutenant Clapot, have been partially preserved; they were all ideographic or symbolic, each

<sup>3</sup> See note at end.

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representing in fact a word, without regard to the number of syllables in it. But as Delafosse says, the roots of the Bamoun language are almost all monosyllabic, so that they can represent, in this simplified form, the majority of the ideas in circulation and of the objects in daily use. Thus was obtained about a thousand different signs whose number and complexity made writing difficult to set down and also to read.

Some of the signs of the first period which lasted up to 1909 are given on page 437.

From 1910 onwards Sultan Njoya strove to simplify his system, which had proved very difficult to use. He came gradually to alter the signs from an ideographic to a phonetic value, which remained constant and corresponded to the sound of a syllable. He achieved a new, syllabic, stage, preceding that of the alphabet by several years. Thus Njoya's invention passed through each of the three stages common to all the principal scripts of history, as the table opposite (FIG. 2) shows.

The literary men of Bamoun hold that their writing has changed four times since the initial stage. They have, in fact, supplied the writer with a first table comprising 419 signs, which may be assigned to about the year 1907, then another of 286 signs, corresponding probably to 1913, then a table of 204 signs in use about 1915, and lastly two tables of 70 signs each, the second one however being much simplified. These last have been used since 1918, and the last (simplified) edition is still in use in the palace of Fouban.

During his reign Njoya encouraged the spread of this writing with all his power. It was taught in the schools, and the leading men and officials used it in their correspondence with him. The Sultan had created government departments to have charge of the records and of the administration of justice; these were made responsible for all documents relating to accounting and government. There can still be seen today, in one of the rooms of the palace at Fouban, a library containing hundreds of laws, decisions, reports of meetings, kept in embroidered skin bags which hang on the walls.

Besides this Sultan Njoya wrote a history of his people and dynasty; composed the philosophy already mentioned; and recorded a large number of legends and fables.

These documents run the risk of being lost. With them will disappear all traces of an extraordinary invention, developed and improved by its author over a period of twenty years. For this reason we must all be grateful to the present Sultan, Seidou Njimolou Njoya, for having presented some of these archives to the National Museums of France.



## THE WRITING OF NJOYA

It was suggested, at the beginning of this article, that Sultan Njoya's achievements had an interest for anthropologists. No one would deny this; but where exactly does the interest lie?

It lies first in the innate intelligence of the Bamoun, a 'primitive' or 'backward' community. (I here use the word 'community' somewhat loosely, without regard to the size of the group so described, to avoid using the word 'race'). Many will be surprised that coloured men should have achieved this. They are unconsciously

























Actual value	Word	Meaning	1907	1911	1916	1918
<i>F, fa, 8</i>	<i>Fama</i>	<i>Eight</i>				
<i>F, fè</i>	<i>Fè</i>	<i>Burn &amp; work</i>				
<i>F, fo</i>	<i>Fom</i>	<i>King</i>				
<i>F, fou</i>	<i>Fou</i>	<i>Measure</i>				
<i>G, ga</i>	<i>Ngè</i>	<i>(Chose-faite)</i>				
<i>G, go, 10</i>	<i>Ngom</i>	<i>Ten</i>				

FIG. 2. BAMOUN SYLLABIC SCRIPT

assuming that it is possible to associate physical and mental attributes and to generalize about them. But it is not permissible to do so. There is no evidence that the mind of the black man is naturally inferior to the mind of a white man (even of a 'Nordic'!), and there is a mass of evidence, including Njoya himself, that points the other way.

The fact that the culture of African communities differs, to use a non-committal word, in so many respects from that of European communities may be due to a number of differentiating factors—

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geographical, climatic, historical, economic, social and such like. These same factors, with others, may also have operated in the past so as to favour the survival of certain physical types of humanity. During the past certain factors of environment have been constantly in operation, factors of a geographical nature for the most part. From out of the interaction between all these factors there has emerged the existing group of human beings in any given region, together with its social organization. The physical make-up of those human beings is, of course, a factor in the whole complex process ; it is no more and no less.

Under certain favourable, but imperfectly known, conditions, the culture of a given community may suddenly change. (To use question-begging terms, it may progress forwards or backwards). The adoption of writing by an illiterate community has many parallels both in our own early history and today in the U.S.S.R. The adoption by a community of an entirely new system of writing, invented by one of themselves, merely goes to prove the absurdity of assuming that any such thing as 'racial inferiority' exists. It is fortunate for humanity that this is so ; for otherwise large groups of it would be doomed to remain 'inferior'. As it is, the evidence of anthropology and history (including our own) emphatically rejects the idea of 'racial inferiority'.

The general phenomenon of what anthropologists call 'culture-borrowing' is familiar, and has of course been long known, though it is not studied by them as intensively as it should be, in view of its practical importance. It is when we come to examine any modern instance closely that it becomes most instructive ; for we are seldom able to subject historical examples to this treatment. In most modern instances of culture-borrowing—such as the adoption of industrial machinery by Japan and the U.S.S.R.—there has been a transference of men and models ; and the resulting installations were, at first at any rate, exact replicas. There is, however, no evidence of any such imitation in the case of the Bamoun ; and it is this fact that differentiates their achievement and adds interest to it. For here we do seem to have got an instance of the borrowing of an *idea*—that of writing in the abstract—which was immediately adapted to the local conditions. Njoya did not take over any ready-made system of writing, but merely the *idea* of writing ; he evolved his own system. No doubt he knew of and had often seen examples of writing ; we are told that an Arabic script was actually in use in his kingdom ; and doubtless European books and newspapers were not entirely unknown. But these systems had little if any direct influence. He began not with literal signs but





PORTRAIT OF A SULTAN (A BAMOUN PAINTING). Ph. Mission Labouret (by courtesy of the Editor of 'Togo-Cameroun')

PLATE II



SULTAN NJOJA ON HIS THRONE

*(By courtesy of the Editor of "Togo-Cameroun")*



## THE WRITING OF NJOYA

with ideographs,<sup>4</sup> which elsewhere have been obsolete for more than three millennia,<sup>5</sup> except in the Far East.

May not the great inventions of humanity have been diffused in just this way? May not writing itself have passed thus from the country where it was invented (whether Mesopotamia or elsewhere) to other countries? A few examples, such as seals or clay tablets, would pass out along some trade-route; eventually one of them would catch the eye of some unknown prehistoric genius like Njoya; he would at once foresee the possibilities, and would evolve a local form of writing. There might be an occasional borrowing or direct imitation of signs, though it is not necessary to assume this. Such a hypothesis is not at variance with what we know of the early scripts of Sumeria and the Indus civilization. For there seems to be enough resemblance between the two scripts to establish some community; and trade connexions between the two regions have been proved. Granted such connexions between the civilizations of the ancient world—and except possibly for the Far East they are all proven—we may infer that the six great scripts of antiquity (those of Mesopotamia, India, China, Egypt, Anatolia and Crete) originated in the way suggested.

So too the Cult of the Dead may have spread as a religious idea. It is quite unnecessary to assume the intermediacy of long-distance carriers of the idea. An original community of custom and cult is all that need be assumed; each region would then develop its own forms. The house of the dead would imitate the houses of the living; in some regions it would take the form of a cave, natural or artificial; in others the cave would come above ground and become a burial-chamber built of big or small stones; in stoneless country it might be a wooden structure; and that, in fact, is just what we do find.<sup>6</sup>

But to return to Njoya. The paramount interest of his achievement lies in the fact that it might appear as an independent invention, if we only knew of it from historical, or even more from archaeological, evidence. What possible parallels could some future archaeologist

<sup>4</sup> An ideograph has been defined as 'a drawing representing not a sound, nor even a word, but an idea'; the oldest known signs were not ideographs but pictographs.

<sup>5</sup> The oldest alphabetic signs are those of Sinai and Syria, which belong to the middle of the second millennium B.C. or before.

<sup>6</sup> I have always held this view, though I have never developed it. (See for instance my remarks in Ordnance Survey Professional Paper No. 6, 'Long Barrows of the Cotswolds', 1922, p. 5). I differ from orthodox diffusionists in my views about the country of origin and in the method of diffusion, as well as from many of their subsidiary theories.—O.G.S.C.



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adduce if he were to dig up one of Njoya's documents? At the most there might be a few resemblances that could better be accounted for by convergence: for the only parallel scripts would be impossibly remote from the Cameroons both in time and space. It would indeed be well worth examining his script to see whether any such convergencies with other scripts do in fact exist. For here at any rate we should know for certain that they *were* convergencies, not borrowings. Such an enquiry would not be of merely formal interest: for it has recently been claimed, on the strength of resemblances between the signs, that the script of Easter Island is derived from the Indus script. Such a suggestion seems quite fantastic, and would hardly be seriously considered, had it not been supported by a distinguished ethnographer. Yet who, with the example of Njoya before him, would not prefer to believe that some long-forgotten Polynesian genius had seen writing and evolved a system of his own?

There is one last point of interest—the rapid evolution of Njoya's script. In a bare twenty years it passed from ideographs to letters. We may doubt whether such a rate of development would have been possible, had not alphabetic signs been already known to the author. It is certain that the actual invention of the alphabet—probably a unique invention—did not occur until at least 1500 years after the invention of ideographs. Njoya however was merely recapitulating the experience of the race, and that perhaps may partly account for his speed.

NOTE. The following is an attempt to translate an extract from one of Njoya's writings:—'Persevere, pursue, follow your predecessors in an effort to reach them. This is the book of the Sultan Njoya of Bamoun, in which he has written the principles of the word of God, selected from the Koran and the Bible. He has brought together everything in this book for fear of God. He who shall read this book and carry out its principles shall enter into the Kingdom of God. He shall overcome sin, and the Musulman law shall be easy for him to observe. It is not right to proclaim the deeds of the prophets before the people. When the people hear the words of the prophets, they seek to do as they do, to imitate them, without giving heed to their own conduct, without seeking to pray that they may be purified of their sin. It is necessary always to seek the means of salvation, and not to imitate the prophets'.







TYPICAL GROUP OF CELTIC FIELDS ON WINDOVER HILL, NEAR EASTBOURNE  
*Reprinted from ANTIQUITY, volume 1, p. 272, plate II*



# The Celtic Field-System in South Britain:

a survey of the Brighton District

By G. A. HOLLEYMAN

**D**URING the past ten years a subject which had hitherto been obscure and of doubtful importance has, by the patient research of a few indefatigable pioneers, come to take a position of first importance among archaeological studies. The history of agriculture, its origins in the fertile crescent, and its subsequent development and spread into Europe is now well-known.

From the neolithic period onwards evidence of corn-growing is not wanting. Grain-rubbers have been found in neolithic flint mines and dwelling sites; charred grain has been recovered from the neolithic camp at Hembury in Devon, as well as in Scotland, and, as Dr Cecil Curwen has so ably demonstrated,<sup>1</sup> irregular corn plots may still be seen associated with neolithic and Early Bronze Age hut-circles on our western moorlands. With the arrival of the first Celtic immigrants during the first half of the first millennium B.C. a more prolific period of corn-growing set in which continued until the end of the Roman occupation. The small squarish fields bounded by lynchets which cover the chalk uplands of our Lowland Zone are now familiar to all. These are recognized as belonging to what is called the Celtic Field-system and many be associated with settlements dating from the Late Bronze Age (1000-500 B.C.) to the end of the Roman period (about A.D. 400). Indeed the Celtic system of agriculture was practised for something like 1500 years, and over a large area, so that it is not surprising that evidence of earlier ploughing is scarce.

Although people are now no longer sceptical about the origin of lynchets, and the Celtic and English Open Field systems with their respective differences are generally accepted, little attention has been

<sup>1</sup> E. C. Curwen, 'Prehistoric Agriculture in Britain', *ANTIQUITY*, 1927, 1, 261-289; E. C. Curwen, 'Air-Photography and Economic History', (*Econ. Hist. Soc.*, 1929); *Report of the 39th Congress of Arch. Societies*, 1931, 30-35.

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paid to the distribution of the former and its association with contemporary settlement sites. Mr O. G. S. Crawford and Mr Alexander Keiller with the aid of air photographs have surveyed numerous lynchet-groups in Wessex with admirable results,<sup>2</sup> and now the Ordnance Survey is publishing a series of maps of Salisbury Plain<sup>3</sup> showing the Celtic earthworks and lynchet-groups as far as they have been recoverable from air observation. In Sussex a small number of individual groups associated with settlements have been surveyed in detail, but beyond these few efforts little has been done.

With this in mind the writer decided to study the Celtic lynchet-groups in the neighbourhood of Brighton, between the rivers Ouse and Adur, with the object of ascertaining (1) their present and original distribution, (2) their area, relative and absolute, (3) their association with settlement sites belonging to the period under review, and (4) the forms of their contemporary field-ways. This area is eminently suitable for this type of survey for the following reasons; (1) it is a segment of the Sussex South Downs, bounded on the east and west by the valleys of the rivers Ouse and Adur, on the north by the Weald and on the south by the English Channel; it has therefore always had well-defined boundaries; (2) from the Late Bronze Age until the coming of the Saxons there has been a succession of settlements on the whole of the South Downs; (3) the Downs consist of chalk which retains evidences of human disturbance for indefinite periods of time; (4) a relatively small amount of downland has been under the plough in recent times, which is the principal reason why so much surface evidence remains.

### DISTRIBUTION

The main ridge of the Downs runs from east to west—on the north descending steeply to the Weald, and on the south sloping gently away in a series of broad, undulating spurs. The average height of these hills is about 500 ft., but the main ridge averages about 650 ft. and at Ditchling Beacon rises to 813 ft. Such uplands as these, situated in the extreme south of our island, are excellently suited to the production of corn, so it is not surprising to find distinct groups of lynchets distributed fairly evenly over the whole area. The majority of them form an almost continuous belt two to three miles wide commencing a

<sup>2</sup> O. G. S. Crawford, *Air Survey and Archaeology*, 2nd edition, 1928; O. G. S. Crawford and A. Keiller, *Wessex from the Air*, 1928.

<sup>3</sup> *Celtic Earthworks of Salisbury Plain*; Old Sarum Sheet. Ordnance Survey, 1934.



## THE CELTIC FIELD-SYSTEM IN SOUTH BRITAIN

little to the northeast of Shoreham and ending at Lewes. It will be seen that these groups with one exception occupy the hills and slopes sheltered from the north by the main ridge, while the latter has for the most part been left uncultivated. A smaller, less complete belt runs across the Downs separated from the first group by the Brighton to Lewes valley. Here again they are sheltered from the north by the secondary escarpment above Kingston by Lewes.

The sizes and shapes of the groups are as arbitrary as those of the hills on which they are found. Some are two miles long covering a north to south ridge and its lateral spurs generally on one side only. Smaller ones may cover single or twin spurs. A notable feature in the larger groups is the presence of lynchets in the small valleys between the spurs, usually above the 300 ft. contour line. As these often run transversely across the valleys from side to side it is clear that the latter were impassable in times of corn-growing.

Considering the large area under cultivation in Romano-British times it is surprising to find any gaps in the continuity of the groups at all, especially as some spurs devoid of lynchets appear pre-eminently suitable for use as arable. These blank spaces may be accounted for by one or more of the following reasons: (1) That the ground was never under the plough in Celtic times; when the surface of the downland is completely unbroken by any irregularity caused by either ancient, medieval or modern ploughing we may assume this to be the case. This land would have been set aside for use as pasturage. (2) That medieval ploughing has obliterated the earlier Celtic lynchets. Two groups of well-defined medieval strip-lynchets exist in the Brighton area, one on Newtimber Hill and the other on Lodge Hill near Piddinghoe; there is a suggestion that the former group may overlie a series of Celtic fields. (3) That modern ploughing has effaced all traces of earlier agricultural practice. This is certainly the cause of barrenness in certain areas, especially near towns and present-day villages. It can be seen that for a radius of one mile round Brighton there is barely the trace of a Celtic lynchet. This paucity is also apparent along the low spurs rising from the coastal plain between Shoreham and Brighton and along the coast between Brighton and Newhaven. In this zone modern tillage has been much more intensive than farther inland, with the result that all vestiges of earlier cultivation have disappeared. Present-day ploughing is rapidly encroaching on several of the groups and several of the larger ones have been split and divided. An attempt has been made to nucleate these isolated fragments and the original



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boundaries have been shown wherever it has been possible to trace them with a fair degree of certainty. (4) That extensive building has covered areas where Celtic lynchets could once be seen. Brighton itself is guilty of this sacrilege, for its ever-widening boundaries have recently encroached on two of the marked groups, *i.e.* Falmer Hill and Tegdown Hill. There is little doubt that these two are by no means the first to suffer and that complete areas have been engulfed within the past 150 years, for Brighton is built on low hills of the type that would lend themselves to agricultural industry.

### AREA

The stretch of Downland selected for this survey covers roughly 65 square miles excluding the river valleys and coastal plain. Of this total at least 23 per cent., or 14.5 square miles, were under cultivation in Celtic times, although the actual area of lynchets extant is 11.45 square miles or 18 per cent. of the whole.

### THE SETTLEMENTS

Having found a lynchet-group and ascertained its boundaries the next problem which presents itself to the field worker is: where was the settlement, or settlements, in which the farmers lived who ploughed the fields, and to which period did they belong? There is no definite rule concerning the positions of villages and hamlets except that the majority favour summits (not on the main ridge) and southern slopes. Variations occur, for a small late 4th-century (A.D.) village<sup>4</sup> was situated on the northeast face of Wolstonbury Hill, and a Romano-British settlement flourished on a northern spur of Falmer Hill. The actual site may be easy or difficult to locate according to whether it has been ploughed over or not. If it has not been obliterated a series of shallow circular depressions marking the positions of pits or huts can generally be made out together with various mounds, banks and other irregularities. Over this disturbed ground burrowing animals have in all probability turned up a quantity of calcined flint and sandstone, potsherds and animal bone. Examples of such well-marked sites, all belonging to the Romano-British period, occur on Thundersbarrow Hill, Highdole Hill near Telscombe, and Buckland Bank.

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<sup>4</sup> *Sussex Archaeological Collections*, 1935, LXXVI, 35-45.



# THE CELTIC FIELD-SYSTEM IN SOUTH BRITAIN

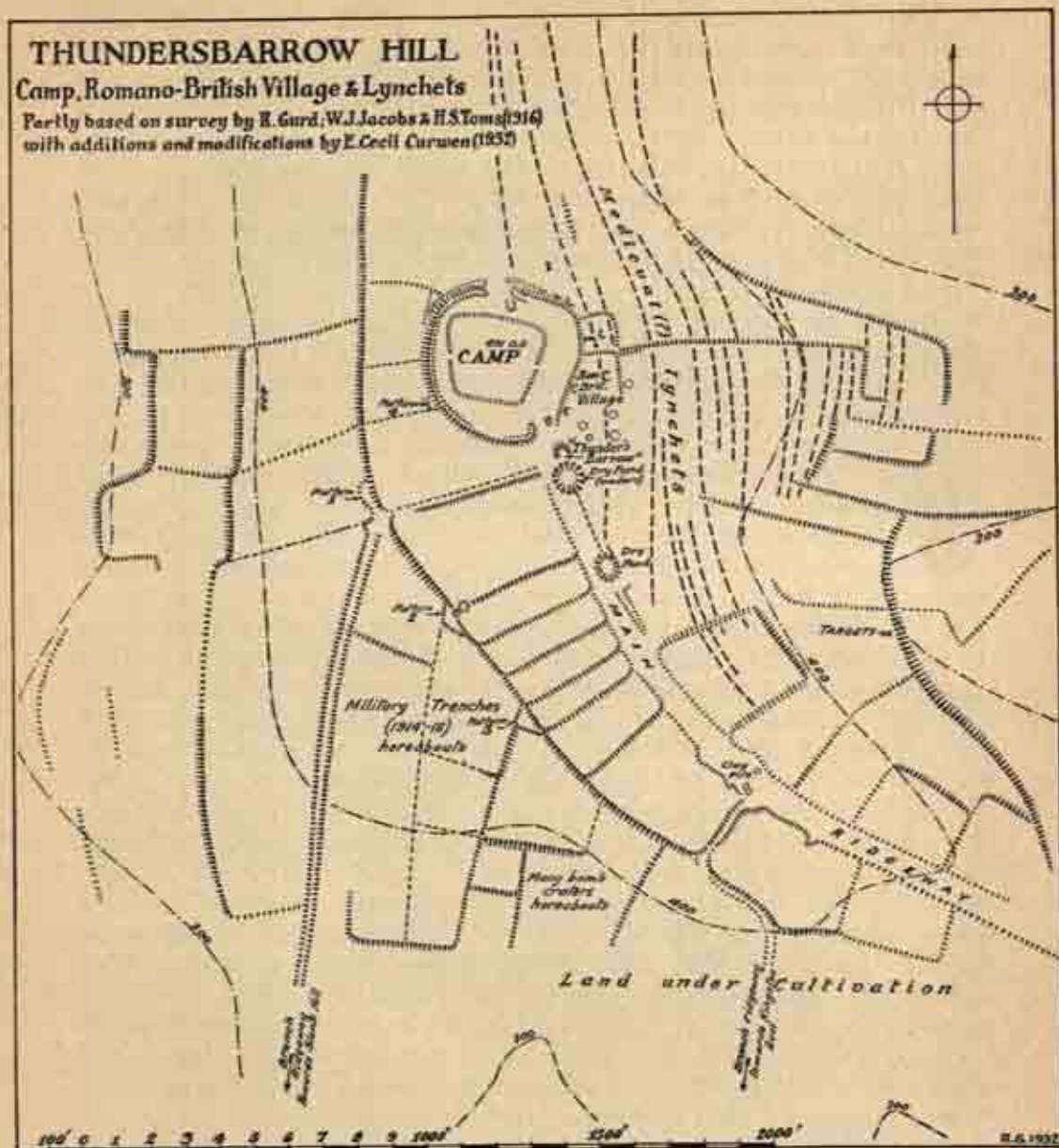


FIG. 1

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On Thundersbarrow Hill<sup>5</sup> (FIG. 1) the village was situated on the eastern fall of the summit and encroached on to the almost obliterated rampart of a small Early Iron Age camp. The surface-appearance is merely that of a series of saucer-like depressions dotted indiscriminately over an area roughly 200 yards long by 100 yards wide. Little surface pottery can be found. A large main ridgeway leads away to the southeast bounded on either side by fields. A double-lynchet road runs southwards down a lateral spur between a network of lynchets. The whole lay-out gives the impression of a highly organized agricultural settlement.

Highdole Hill (FIG. 2) site exhibits similar features. Roughly twenty circular pits and depressions lie close together on the summit in an area strewn with La Tène III-Romano-British sherds. There is an appearance of order in the arrangement of the huts, which seem to have been erected on either side of a roadway running through the village from northeast to southwest. A double-lynchet road runs away to the northeast and a second, rather mutilated example, enters from the southwest. The settlement is in line with fragments of a well-defined bivallate road a few hundred feet to the north, which possibly connects with another known village in that direction.

The Buckland Bank site<sup>6</sup> (FIG. 3) is the most complete of the three. The position of the village is marked as in the previous ones by shallow depressions and a general unevenness of the surface. Sherds, Roman tiles and calcined flints may be found within this disturbed area. A fine bivallate road runs from the northwest, terminating close to an earthen circus which was probably the meeting place or moot of the community. Across the valley to the west, on Balmer Huff, an adjacent spur, there is evidence of another village of the same period. Although ploughed level, a large amount of pottery, calcined flints and Roman tile is distributed over a wide area. In 1849 the cemetery<sup>7</sup> of these neighbouring villages was discovered in the head of the valley between them. It was a circular area, 135 ft. across, in which cinerary urns containing cremated remains had been placed at a depth of 2½ ft. Between the two sites and extending for a considerable distance to the north and south there is a finely developed stretch of Celtic fields. Thus we have an even more perfect example of the appearance of the Downs during the Roman period. In one big group of fields we see

<sup>5</sup> *Antiquaries Journal*, 1933, XIII, 109-51.

<sup>6</sup> *Brighton and Hove Archaeologist*, II, 29-40; III, 62-74.

<sup>7</sup> *Sussex Archaeological Collections*, 1877, XVIII, 65.



**DISTRIBUTION OF THE CELTIC FIELD SYSTEM  
and contemporary Settlements in the  
BRIGHTON AREA**

**Legend:**

- Red shaded area: Visible extent of Celtic system
- Red shaded area with dots: Probable original extent of Celtic system
- Red dot: Site of small settlement
- Red dot with cross: Site of settlement of uncertain size
- Red dot with cross: Village site
- Red circle: Hill Fort
- Vertical hatching: Modern Towns
- Plus sign (+): Present day Villages

**Settlements:**

- A Roman Villa
- A Late Bronze Age
- B Neolithic-Late Iron
- C La Tene III
- D Romano-British

**Scale:** 0 to 10 Miles

5 MILES





## THE CELTIC FIELD-SYSTEM IN SOUTH BRITAIN

two, or perhaps three, villages with their contemporary roadways, their circus or moot,\* and their cemetery.

On Plumpton Plain (FIG. 4) two sites\* of an earlier and rarer type

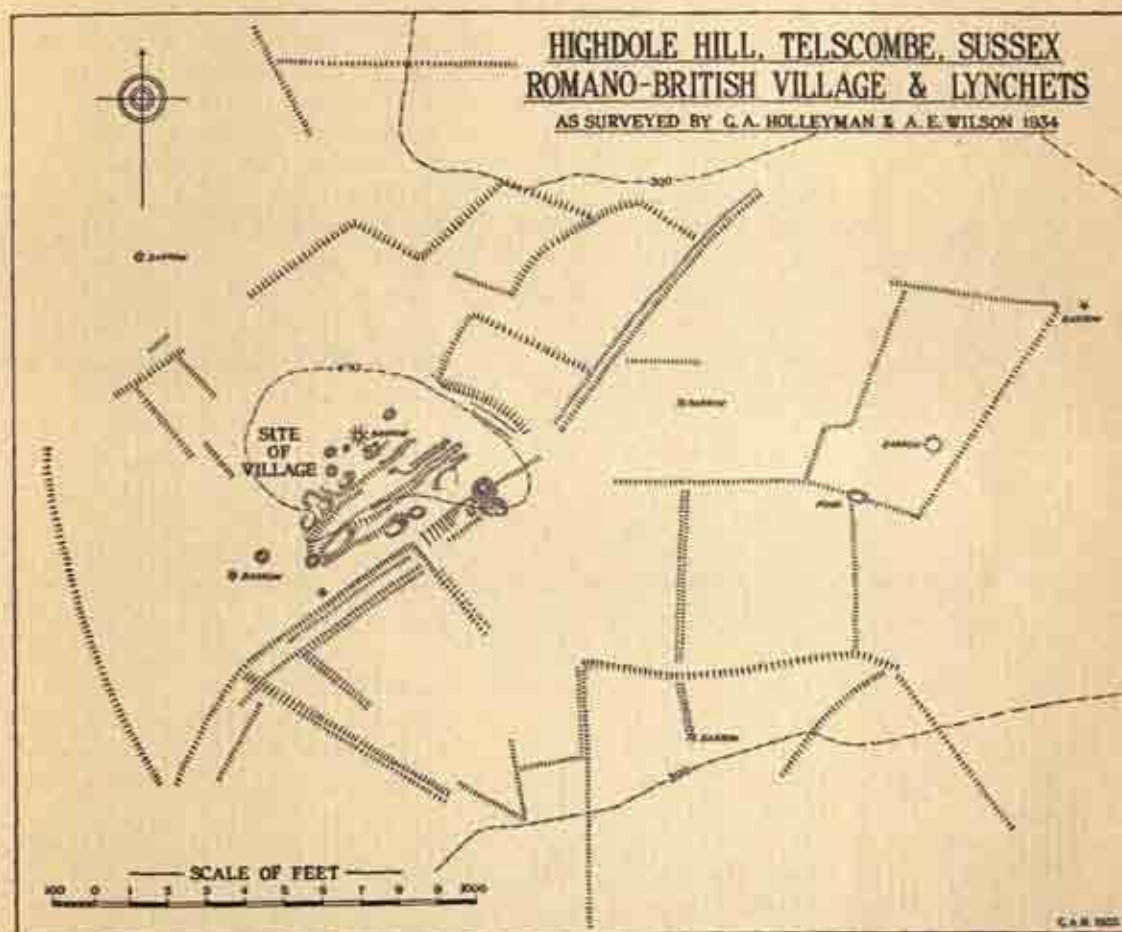


FIG. 2

have been discovered in connexion with lynchet-groups. Site A comprises four compounds or enclosures belonging to the Late Bronze

\* Opinions differ about the purpose of these earthworks; the moot theory is not generally accepted.—EDITOR.

\* G. A. Holleyman and E. C. Curwen, 'Late Bronze Age lynchet-settlements on Plumpton Plain, Sussex'. *Proc. of the Prehist. Soc.*, 1935.

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Age in which the positions of circular huts and water catchments were found. Integrally associated with them is a small group of lynchets enclosing typical Celtic fields. Across Faulkner's Bottom to the west two isolated enclosures of the same type are situated on the eastern slope of Streat Hill. They were made on the side of an ancient terrace-way and each has a small group of lynchets close by. Site B was found on a small lateral spur a quarter of a mile to the southeast of site A. The only surface-evidence of its existence was a quantity of small gritty sherds close to a low bank and ditch running across the spur. It was occupied from the Late Bronze Age to the Hallstatt period and then deserted. The fields contiguous with site A lie to the west but a larger group of well-defined lynchets extends to the south. There is no definite proof that these are associated with site B, but as no trace of Romano-British occupation has been found in the vicinity, in spite of careful search, this is highly probable. In the southwest corner of the same plan two more sites are marked on Stanmer Down. The first, in the extreme southwest, comprises a small cluster of circular depressions in and about which coarse, gritty sherds of Hallstatt character with fragments of calcined flint and sandstone are strewn. A more complete survey of the spot with the surrounding lynchets must be made before the two can be chronologically related. The second site is marked only by a fair number of Hallstatt sherds and the ubiquitous pot-boilers. As this is within a lynchet-group, but is close to where present-day ploughing has encroached, it is impossible to tell whether the Celtic or the modern fields hide the positions of the huts.

The field-worker should be wary of marking a certain spot where a few sherds may be picked up, with no other surface-evidence, as the site of an Early British village. Only when a considerable quantity is found in conjunction with burnt flint and other objects, and in close proximity to a series of Celtic fields, can one be certain of the place having been occupied for a definite period of time. Pottery-dating may even give the approximate period of occupation if the surface sherds are sufficiently large. If the pits have been levelled their exact whereabouts can often be detected by 'bosing' or 'percussing' the ground with a metal rammer. This implement is the usual type used by navvies and is invaluable when the presence of pits is suspected yet no visible trace is apparent. A simple change of note occurs when disturbed ground is thumped after solid ground has been worked over.

Four Early Iron Age hill-forts lie within the area of our survey, viz: Thundersbarrow, the Devil's Dyke, Wolstonbury and Hollingbury.





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They have all been examined with the spade except the Devil's Dyke. In none was there any evidence to suggest the presence of a settlement or any prolonged occupation; rather were they places of refuge and retreat in times of danger. Although built during the early part of the Celtic corn-growing period no lynchets appear integrally associated with them. The fields surrounding the Thundersbarrow hill-fort were formed long after the latter had fallen into disuse.

### THE FIELD-WAYS

It has not been possible to mark associated field-ways on the distribution-map although all the known varieties occur. A fine example of a bivallate<sup>9</sup> road is shown in the Buckland Bank plan and another equally good but shorter stretch runs between Highdole Hill and Pickers Hill north of Telscombe. Double-lynchet roads are most common though few long stretches remain; the finest remaining portion is to be seen on the Brighton and Hove golf course and is believed to be part of Port's Road running from Southwick, via Hangleton, to the Devils Dyke.<sup>10</sup> Other forms noted are the terrace-way, generally regarded as Roman, and the hollow-way or sunken road. An example of the former runs along the eastern face of Wolstonbury Hill to terminate close to a Romano-British village of the late 4th century. From the numerous fragments of field-ways extant it is evident that a well organized road-system existed on the Downs during the whole of the Celtic period.

Three cross-ridge dykes were found but their exact relationship with nearby settlements is difficult to ascertain. Not one appears to be in a position of strategic importance, and for defensive earthworks their length, height, and situations are totally inadequate. The three examples noted were, in the writer's opinion, used either as tribal boundaries or to prevent cattle from straying or being stolen.

### CONCLUSIONS

This survey has shown with great clarity the extent to which the chalk uplands were inhabited and cultivated during Celtic times and particularly during the Romano-British period. We see large tracts

<sup>9</sup> A bivallate road is a slightly hollowed track between two low banks. A double-lynchet road is a track along a hillside between a positive lynchet above and a negative lynchet below. Terrace-ways have, as a rule, been engineered and are not the result of ploughing.

<sup>10</sup> *Brighton and Hove Archaeologist*, III, 28-41.



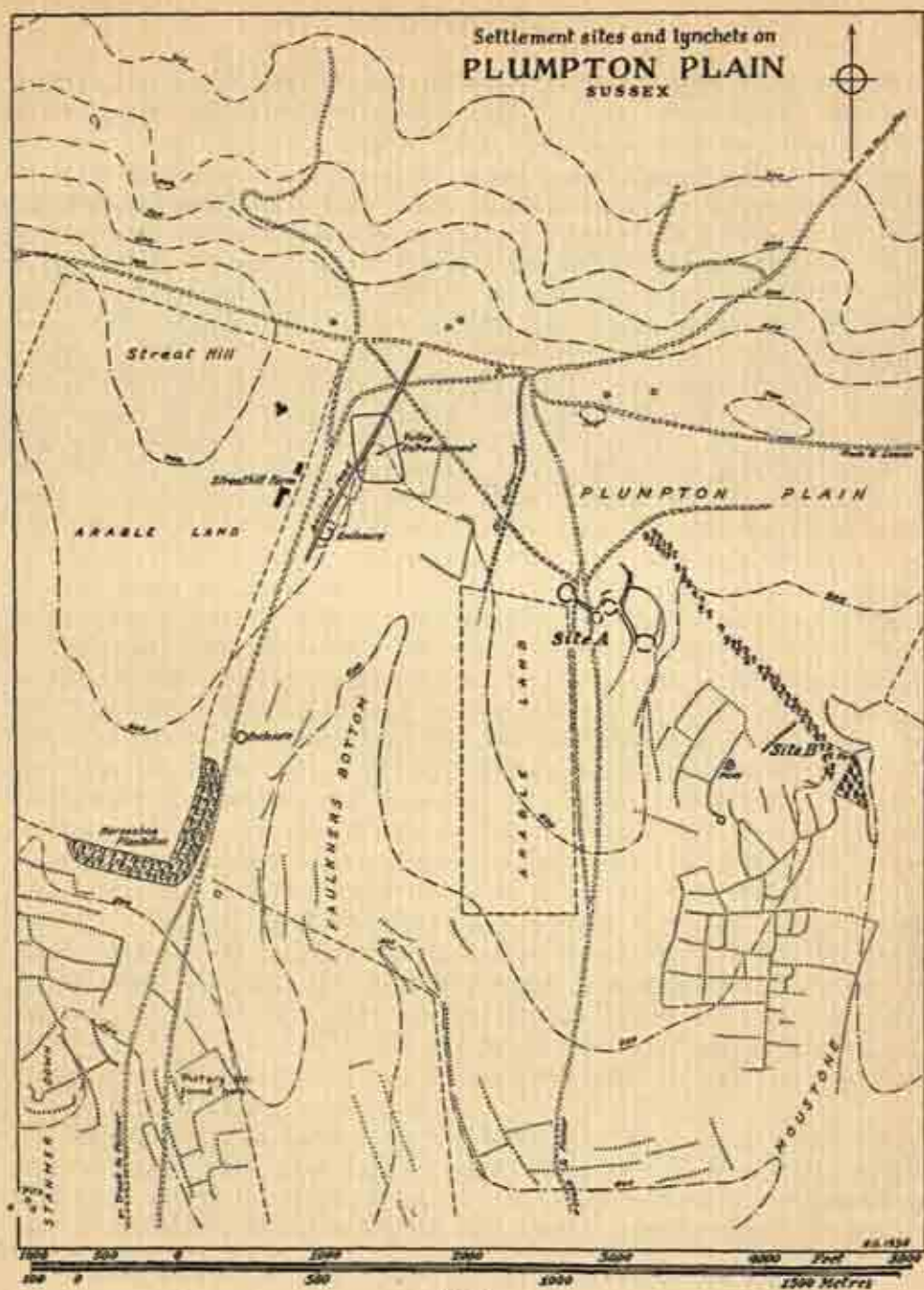


FIG. 4

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of downland devoted to corn-growing, each controlled by one or more villages, hamlets or farms. There is little doubt that many of the settlements were occupied for long periods, as on Thundersbarrow, where the village had its inception in La Tène III and was tenanted until the end of the 4th century A.D., or as on Park Brow,<sup>11</sup> near Worthing, where there was a continuous succession of settlements from the Late Bronze Age until the coming of the Saxons. It also shows that conditions were peaceful and prosperous and confirms the evidence of classical writers that South Britain was an important centre of corn production.

In conclusion the writer wishes to thank the instigator of this survey, Dr E. Cecil Curwen, for kind assistance and encouragement throughout, also those fellow field-workers whose observations and discoveries have considerably added to its completeness.

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<sup>11</sup> *Archaeologia*, 1927, LXXII, 1-40.



# Britain in the Dark Ages

by J. N. L. MYRES

MAP OF BRITAIN IN THE DARK AGES. South Sheet. Scale 1:1,000,000. Ordnance Survey, Southampton. 1935. Mounted and folded, 5s 6d, other styles available.

ALL that learning can extract from the rubbish of the Dark Ages is copiously stated by Archbishop Usher in his *Britannicarum Ecclesiarum Antiquitates*, wrote Edward Gibbon in a moment when the conflicting emotions of irritation and complacency had fought one another to a standstill in his heroic breast.<sup>1</sup> And while it would be unjust to censure the great historian for an exaggeration at once pious and harmless, it is legitimate to observe that neither he nor the good Archbishop had thought of plotting 'the rubbish of the Dark Ages' on a map. That task has at last been tackled by the enlightened Direction of the Ordnance Survey; and although we can hardly be rash enough to assume that its completion may have moved Gibbon in the Elysian fields to reconsider the propriety of his preposterous dictum, it is at least a matter for sincere congratulation in more mundane circles.

The work has necessarily been one of extreme difficulty, a difficulty which can perhaps best be appreciated by a comparison with the corresponding Map of Roman Britain, for the existence of which both teachers and students of the period have blessed the Ordnance Survey since its first appearance in 1924. For this the archaeological material was of paramount importance, and its classification under well defined headings—Towns, Villas, Villages, Forts, and so on—each with its appropriate symbol, was a matter which, if certainly laborious, did not provide too many insoluble problems once the main principles were laid down. At least the great gulf fixed by archaeology between the cultural remains of the Roman period and those of Iron Age Britain on the one side, and Anglo-Saxon England on the other made it comparatively simple to decide what deserved a place on the map and what did not, while the paucity of literary material gave the result a homogeneity, which could hardly have been expected had it been necessary to cull the evidence from a greater variety of sources.

In every one of these ways the Dark Age Map was a much more awkward problem. Although an upper limit of date was readily made with the 'departure of the Romans', it raised inevitably the question how much of the Romano-British past should legitimately be included

<sup>1</sup> *Decline and Fall*, ed. Bury (1925) IV, p. 67, n. 23.



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as necessary to the understanding of the new society. Roman roads, for example, were certainly available for transit in Saxon times to at least the extent to which they were available later in the Middle Ages: the ruins of Roman towns, even of those which were never reoccupied, must have been conspicuous objects in the Anglo-Saxon landscape, and sometimes influenced its later administrative divisions. Is a picture of Saxon England complete without them? The principle here adopted has been to include those only which are mentioned in sources of the period or have produced archaeological remains of Dark Age date: a principle which could perhaps be criticized for undue austerity, for it has left the Saxons with no roads at all but the Kentish Watling Street, the Icknield Way, and the Berkshire Ridgeway, and some towns such as Colchester are conspicuous by their absence. It is reassuring to know, however, that a more wholehearted attempt to portray the roads of the Saxon period will be made on the promised map of the period 871-1066.

This brings us to a further difficulty. The lower limit of date has been fixed at 871, the beginning of the reign of Alfred. The Dark Age, like any other Age, must of course end somewhere, and there are better reasons in England than in Europe as a whole for ending it at a date which excludes the greater part of the Viking period although in the opinion of many scholars that period marked the darkest hour of all. For England, uncheered by the premature dawn of the Carolingian Renaissance, could the better react to the social and political devastation caused by the Norsemen with the first glimmerings of national unity. And the accession of Alfred marks unquestionably the beginning of this new epoch. None the less there was no sudden change, and the choice of the year 871 indicates really no more than a desire to limit the picture of Saxon England to the pre-Danish phase, a fact clearly shown by the deliberate exclusion from the map of such Viking names as were in evidence before that date.

Even so, however, the period represented is both longer and far less homogeneous than that shown on the Romano-British map. In the Lowland Zone we see the pagan cemeteries and certain place-names which belong to the age of chaos and conquest cheek by jowl with the churches and crosses of the eighth and ninth centuries, symbols of a settled and more civilized community. The Wansdyke, which can only represent a political situation already obsolete before the earliest days of the historic Wessex, is marked with the same symbol as Offa's Dyke, the expression of the great era of Mercian expansion in the



## BRITAIN IN THE DARK AGES

eighth century. The Dark Age Map thus hardly tells its story by itself as does the Roman Map : and for the full appreciation of it considerable knowledge both of the period and of the sources is essential. Whether it would be possible to make its lessons more obvious by printing the sites of the pagan period in one colour, and those of the Christian in another, while inevitably leaving many of the place-names doubtful, has probably been discussed already in the proper quarters : it would certainly increase very greatly the value of the map as an educational weapon if something of this kind could be attempted in the next edition.

One further suggestion may be offered in the matter of classification. The symbol for churches has been limited to those sites 'where it is certain upon archaeological grounds that such existed'. Monasteries, on the other hand, though so essential a feature of the period both in the Celtic and the Saxon lands, are not distinguished either as churches or by a special sign of their own, although in fact the more important of them do appear disguised as 'Other Places'. This leads to some odd and rather unhistorical impressions (Wirksworth or Britford, for example, as churches, while Repton, Oundle, Abingdon or Whitby are just 'Other Places'). The reason given for the exclusion of a symbol for monasteries, that apart from the known sites 'many others . . . certainly existed, though unrecorded', is a counsel of perfection which has fortunately not been followed in dealing with other classes of the evidence. If the pagan cemeteries or even the crosses had been excluded because those known represent only a tithe of what once existed, the map would have been reduced to an almost worthless blank. And the monasteries certainly deserve a special symbol, accompanied by the date of foundation where known, if only because their distribution in lowland England is a matter of the greatest interest : it is one of the few clues that we have to the gradual opening up of areas of secondary settlement after the first rush of the conquest was over.

Here and there, as is inevitable in a pioneer work of this kind, some sites which deserve inclusion seem somehow to have slipped off the lists. Take Yorkshire for example. Even if the Anglian girdle-hangers in the Aldborough Museum hardly demand the insertion of a pagan burial at Isurium Brigantum, there are undoubted cemetery groups from Robin Hood's Bay and Scampston Park, Rillington, in the Yorkshire Museum ; there are the fragments of the cross from Cundall, now at Aldborough Manor, and there is the historically interesting



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place-name in Dera Wuda which Bede associates with the monastery of Beverley.<sup>1</sup>

But to emphasize such criticisms would be to do less than justice to the singular merits of the map. It illustrates in striking fashion the differences which in this as in every age have held apart the Highland from the Lowland Zone of Britain. The use of Mr Nash-Williams' card-index of the Welsh inscribed stones has given for the first time a picture of the twin *foci* of dark age Christianity in the northwest and south of the principality. The inclusion of all boundary ditches and travelling earthworks, many of them hitherto recorded inadequately or not at all (irrespective of a proved origin in this period, which most of them at present lack) is a generous and welcome feature. The recent results of place-name study have made it possible to identify and localize a number of those folk-names, such as the Sweordora and Herstingas of the Tribal Hidage, or the Sunningas of the Chertsey charter, which covered England before the later administrative units of Shire and Hundred drove most of them out of memory: some of these however, like the early *provinciae* of Kent, or the Faenn-ge and Geginas of Essex seem rather unaccountably missing.

Other classes of the place-name evidence it has not been found possible to include on the main map. For this, however, compensation is provided in the form of a smaller map in the Introduction showing the distribution of names in -ing (singular and plural), together with those containing names of heathen gods or sanctuaries. The '-ingas' names, it is generally agreed, belong to the oldest stratum of Teutonic nomenclature in England, but historians have always felt chary of utilizing them as direct evidence for penetration in the heathen period, because there seemed no sure means of knowing when they ceased to be formed. Not the least valuable part of the present publication is that side by side and uniform with the map on which they are shown has been printed another showing the distribution of the pagan Anglo-Saxon cemeteries. Historians are thus provided with the raw material for comparing, both in the mass and in detail, the distribution of the '-ingas' names with that of archaeological material all of which can be dated with confidence before the middle of the seventh century. If the two distributions are found to coincide it will be very strong evidence to suggest that the place-names too can be safely dated to the heathen period.

The two distributions are markedly similar in their general lay-out.

<sup>1</sup> H.E. v. 2.



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Both cover roughly southeastern England within a line from Southampton to Gloucester, and there northeastwards to Whitby. Both show beyond these limits a stray or two near the Tees valley and an isolated example on the Northumberland coast; and if there are four '-ingas' names in Lancashire unmatched by cemeteries, there are four cemeteries in South Wilts and one in Somerset beyond the limits of the '-ingas' names in this direction. And it so happens that all these five are known to fall late in the archaeological list.

We may therefore set our doubts at rest as to the general contemporaneity of the two series. It would seem as certain as anything can be in these dark centuries that the '-ingas' names as a whole were formed in the pagan period or not much later. This qualification is perhaps advisable because a closer inspection of some areas confirms the feeling raised by the Lancashire '-ingas' (perhaps a product of Northumbrian expansion under Ethelfrith or Edwin in the first half of the seventh century) that the formation of such names may have continued in some districts rather longer than the practice of pagan burial. In Sussex, for example, the pattern of the pagan cemeteries, practically confined to the strip between the northern escarpment of the South Downs and the sea, is definitely more primitive and limited than that of the '-ingas' names which extend also freely along the base of the South Downs and for some distance up some of the rivers. If we say that most '-ingas' names were formed in the fifth and sixth centuries, a few after 600, and none after 700, the limits are wide enough to be reasonably safe. And something of value has been gained for the historian.

But within the general similarity there are some curious differences of distribution. A more pronounced concentration of the cemeteries than of the '-ingas' names in the major river valleys may merely reflect once more the slightly earlier emphasis of the former in date, but there are large areas such as Essex which though full of '-ingas' names have produced little or no archaeological material, and conversely there are others such as the Cambridge region, the Oxford district, Lincolnshire or the valley of the Middle Trent, where a mass of early cemeteries finds little or no echo on the place-name map. Comparing the two sets of evidence as a whole, it is indeed a somewhat striking fact that if, as we assume, the men who formed the '-ingas' communities were contemporary with those who found their last resting places in the pagan cemeteries, there should be so few instances in which we can actually identify the two. There are some 200 '-ingas'



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place-names marked on this map, but not more than a dozen of these, of which Reading (Berks) and Kettering (Northants) are the best known cases, are also distinguished as the sites of pagan cemeteries.

A few specialists have long been dimly and rather uneasily conscious of these odd divergencies, particularly perhaps of the startling contrast between the cemeteries-without-*'-ingas'* of the Cambridge region and the *'-ingas'*-without-cemeteries of Essex, but with the publication of the Dark Age Map the specialists lose willy-nilly their monopoly of the awkward secret. And with the relevant facts thus conveniently and for the first time at our disposal, it may be worth while to draw attention to some points which should help to put the difficulty in its true perspective, and to indicate its historical interest.<sup>2</sup>

It goes without saying that any tendency to magnify the problem should be sternly resisted. Neither group of evidence is or ever will be complete: both at best are but representative selections of the material which was once in existence or may still be found, and some of the discrepancies in distribution are doubtless due to the fact that the selection being to a large extent accidental is bound here and there to be misleading. However, if we turn our thoughts again to the contrast of Cambridge and Essex, it is clear that chance alone will not supply the answer.

There are ways too in which some of these chance discrepancies due to paucity of evidence may be discounted. To take, as has here been done, only the *'-ingas'* without considering the *'-ingahams'* and certain other compounds, can be justified on grounds of prudence, for many of these forms, notably the *'-ingtons'*, are notoriously difficult of interpretation and some of them are irrelevant to the discussion, not being really based upon *'-ingas'* forms at all. All, even the *'-ingahams'*, which have the strongest claims to antiquity, are perhaps later in date than the simple folk-names uncompounded. But a compound whose early forms show a basic *'-ingas'* stem is, whatever its own date, evidence for the previous existence of the *'-ingas'* group in question. And where such compounds occur within the part of England in which such folk-names are common, there is no reason for rejecting their

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<sup>2</sup> Mention must here be made of the great debt which all students of this topic and the compilers of the map owe to Professor Ekwall's *English Place-names in -ing* (Lund, 1923). He did not however attempt to date his material by comparison with that of the archaeologists, and his suggestion that the *'-ingas'* names as a whole are 'not much later than c. 500' (p. 115) has not commanded universal assent for it depends upon the acceptance of the traditional story of the West Saxon advance from Southampton Water. See below, p. 464, for further treatment of this aspect of the question.



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plain evidence merely because they have at a later date received a termination which was then in vogue. Bensington near Dorchester-on-Thames, for example, is the 'tun' of the 'Banisa ingas', and we have the less right to disbelieve in the existence here of such a folk at an early date when we remember that in later days the village was the centre of one of those great estates which like Sonning and Reading, further down the river, seem to have carried on into the Middle Ages not only the names but the geographical outlines of a folk settlement in the Thames valley going back to the earliest times.<sup>4</sup> And if we can put Bensington and many others like it on the '-ingas' map, some of the blank areas where cemeteries alone exist will tend to disappear. The process is one which must be applied with the utmost caution and the fullest safeguards, but the principle behind its application is perfectly sound.\*

Even so however the problem will remain, and perhaps the best way of tackling it is to consider the relative survival value, so to speak, of cemeteries and folk-names, the two classes of our evidence. Pagan Saxon cemeteries, as the excavated instances repeatedly show, have stood a greater chance of surviving more or less undisturbed until the days of systematic record in the eighteenth and nineteenth centuries, where they lay out in the open fields or on the permanent pasture of the Downs away from the villages or towns of later times. Many it is true have been discovered during building operations but in most cases such building has been on ground hitherto open, not within areas already filled with houses and shops. To put the matter in a different way, it is in those regions in which there have been most changes in the detailed distribution of population since pagan times that our Anglo-Saxon cemeteries will tend to be most in evidence. Where village life has been undisturbed since the days of the first settlers, especially perhaps where the Christian has directly succeeded to the pagan graveyard, there in countless cases the ceaseless and necessary routine of the village sexton must have destroyed piecemeal and unnoticed the good work of his heathen predecessor.

But those are precisely the villages in which continuity of occupation is most likely to be typified by the retention of the earliest types of place-names. Such names are tender plants, especially in the early days when, as was the case with the '-ingas', they were not really the

<sup>4</sup> Stenton, *Introduction to the Survey of English Place-Names* (1924), pp. 41, 172.

\* A distribution-map of names in -ingham will be given in future editions of the map.—O.G.S.C.



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names of places at all but the names of the folk who lived there. If one fine day in the sixth or seventh centuries the huts of the Woccingas were burnt by the raiding bands of Ceawlin or Penda, and the survivors decided to start again some distance away from the old site, the new village might turn out to be Wokingham, but it might equally well be Drayton or Clayton and the folk-name would then be lost for good. If we are right in thinking that new names in '-ingas' were not formed in any number after the seventh century, then all civil disturbances in that and later times in southeastern England are likely to have resulted in wastage of such primitive names, and the wastage will be the greater in proportion to the extent and frequency of such commotions in the different districts. Meanwhile the cemeteries of the communities which altered their positions and lost their early names acquired by mere disuse a survival value higher than those of the communities which remained. The two distributions will thus tend in extreme instances to exclude one another.

It is easy to try this explanation on the ground and to see how in some of the crucial cases it will work. Everything goes to show that the Cambridge region, so fertile in remains of the pagan period, so poor in early place-names, was a battle ground in the seventh century between East Anglia and Mercia in their struggle for the control of the Middle Angles.<sup>5</sup> Cambridge itself the natural centre of it, and surrounded with early cemeteries, was a *civitacula quondam desolata* when the monks of Ely came sailing by in 695 in search of a stone coffin for St. Etheldreda.<sup>6</sup> Equally desolate no doubt were many of the surrounding communities whose cemeteries we now know by the names of their successors: Girton, Barrington, Little Wilbraham, or Haslingfield. The Oxford district was similarly in the late seventh and eighth centuries the scene in which Wessex fought a rearguard action with the rising power of Mercia, and it is here between the Chilterns and Fairford that the trail of '-ingas' names up the Thames valley to the Goring gap gives place to a striking group of early cemeteries. It is customary to explain the scantiness of primitive place-names in Lincolnshire by reference to its thorough Scandinavianization later on, and no doubt there is much truth in this. But here too we may remember that in the seventh century Lindsey repeatedly changed hands in the struggles between Mercia and Northumbria and is not likely to have suffered these changes without severe

<sup>5</sup> C. Fox, *Archaeology of the Cambridge Region* (1923), pp. 245, 295.

<sup>6</sup> Bede, H. E., iv, 19.



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social dislocation.<sup>7</sup> And finally the case of the Isle of Wight is not without interest. The island has produced more archaeological evidence of its early Jutish population than the neighbouring parts of Hampshire, but on the '-ingas' map it is a blank. We remember without surprise that Caedwalla of Wessex in 685-6 attempted to exterminate the island Jutes and to replace them by West Saxon settlers.<sup>8</sup>

The regions in which '-ingas' names are most frequent at the present day are Sussex, Kent, Essex and East Anglia. Sussex was described by Eddius early in the eighth century as 'a province which has impregnably resisted the attacks of other provinces owing to the difficulty of the terrain and the density of the woods', and there is no reason to doubt the essential justice of his description.<sup>9</sup> Hemmed in between the weald and the sea it was the most isolated and the least disturbed of the Saxon Kingdoms, and it retains by far the highest percentage of archaic names. Kent, in early times the richest and most stable kingdom, seems to have been but little affected by external troubles except for an incursion by Ethelred of Mercia until late in the eighth century, and soon afterwards passed easily and permanently under the benevolent control of Wessex. East Anglia proper can hardly have suffered as severely from her losing struggle with Mercia as did her outlying dependencies in the Cambridge region and round the Southern margin of the Fens. It would be foolish to claim that the present argument solves the standing puzzle of the blank made by Essex in the archaeological map of sixth-century England; but here too the difficult forest country, and the poverty of its scanty population, may partly explain both the uneventful character of East Saxon history and the survival of so many of its ancient names.

Our survey may thus be summarized by suggesting that the archaeological and the place-name evidence plotted on these maps forms a consistent and intelligible whole, for its two halves are mutually complementary. To depend on the negative evidence of either by itself is liable to be misleading: but taken together they provide a reliable test of the areas most thickly settled by the Anglo-Saxon people in the earliest phase of their history. And if following this clue we try to visualize the settlement by super-imposing the one distribution on the other there will be a further moral of some interest to be drawn.

<sup>8</sup> Bede, H. E., IV, 16.

<sup>9</sup> *Vita Wilfridi*, c. 41.

<sup>7</sup> Plummer's note on Bede H. E. III, 11, conveniently summarizes these changes in the political allegiance of seventh-century Lindsey.



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In England south of the Trent, and within the limits traditionally associated with Saxon occupation in this period, four areas will show up as relatively free from either type of evidence: the Fenland bordering on the Wash; the country northwest of London between the Lea, the Chiltern escarpment and the Middle Thames; the Andredesweald; and the traditional homeland of the kings of Wessex, the chalk uplands of Hampshire, Wiltshire, and South Berks. It is legitimate to infer from the unanimity between the two distributions that in these areas Anglo-Saxon folk had not yet driven their roots firmly into the soil. What were the reasons? We need not go further than Felix's eighth-century *Life of S. Guthlac* to appreciate the Saxon horror of the Fens:<sup>10</sup> and a phrase in which Eddius at much the same period speaks graphically of the outlaw's home in *desertis Ciltine et Ondred* may serve in part at least to explain two others,<sup>11</sup> but what are we to say of the traditional conquests of Cerdic and Cynric? If we follow Mr Hodgkin in his recent opinion that they were 'clever and adaptable men who saw that the depositing of weapons and jewellery with the corpses was an unnecessary extravagance',<sup>12</sup> we must, it appears, believe also that they imposed a ban on the adoption of '-ingas' names by their followers, an action in which the qualities of cleverness and adaptability are not so immediately apparent. Were they perhaps leaders without an army? Or have they derived a reflected and undeserved glory from the deeds of their remote and more substantial descendants? There is no sure answer at present to these questions: one may note only that, if our line of thought has any value, the really damaging thing about the story of the West Saxon heroes is not the paucity of the archaeological evidence in Hampshire and Wiltshire, of which so much has been made, but the almost complete absence in a land so little disturbed by later invaders of the earliest types of place-names.

The moral of this over-lengthy discussion is two-fold. First we should like to see the early place-names on the next edition of the Dark Age Map printed if possible in the same colour as the cemeteries. And second, gratitude for the present publication far outweighs any criticism that may be made against its details. In the Dark Ages more perhaps than in any other period maps are the only sure basis of historical knowledge, and this is the first attempt at a complete map that has been made. Now at last, thanks to the Ordnance Survey, the historians of these dark centuries can get on with their job.

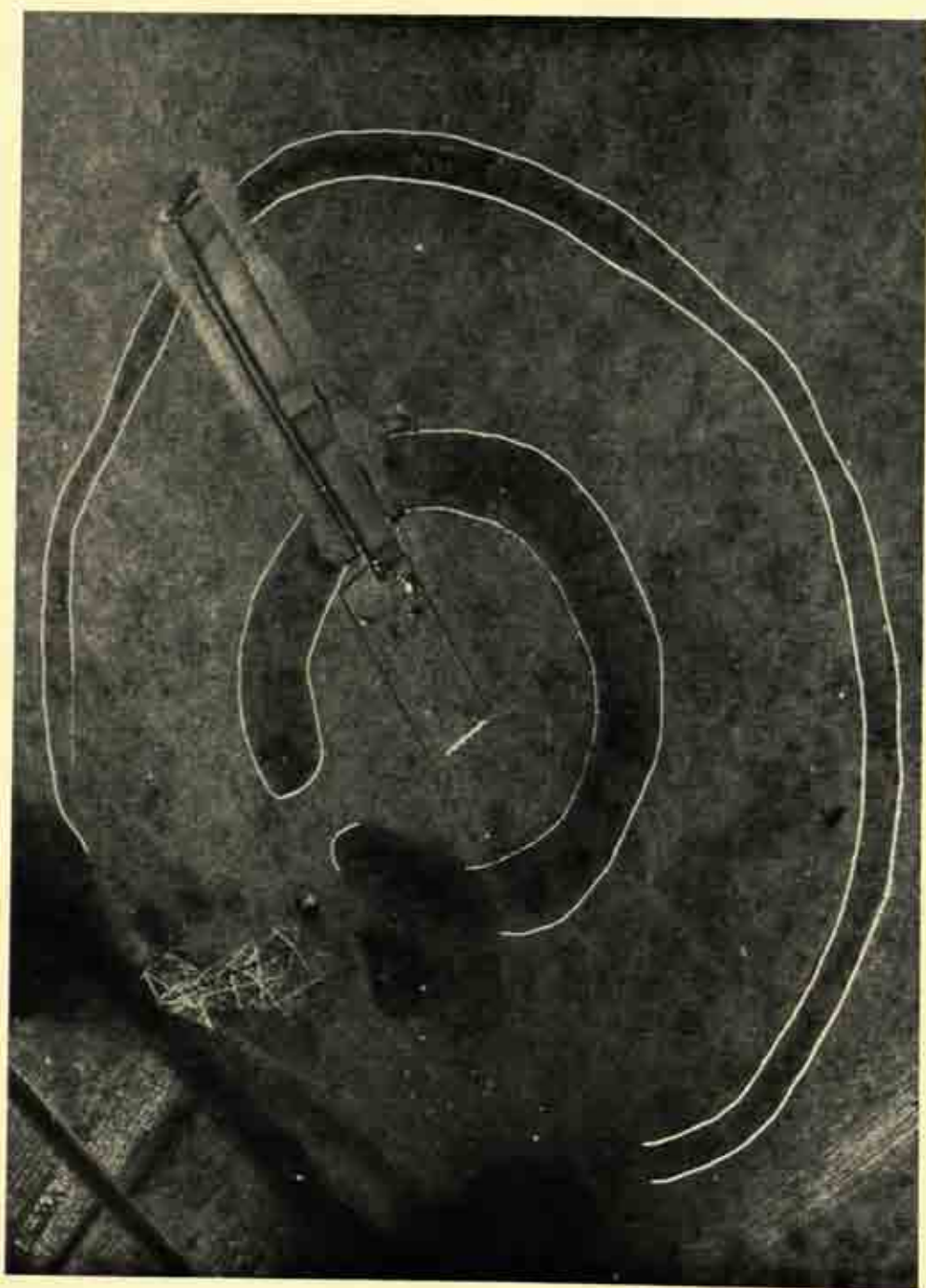
<sup>10</sup> See *ANTIQUITY* (1934) VIII, p. 191.

<sup>11</sup> *Vita Wilfridi*, c. 42.

<sup>12</sup> *History of the Anglo-Saxons* (1935), I, p. 131.

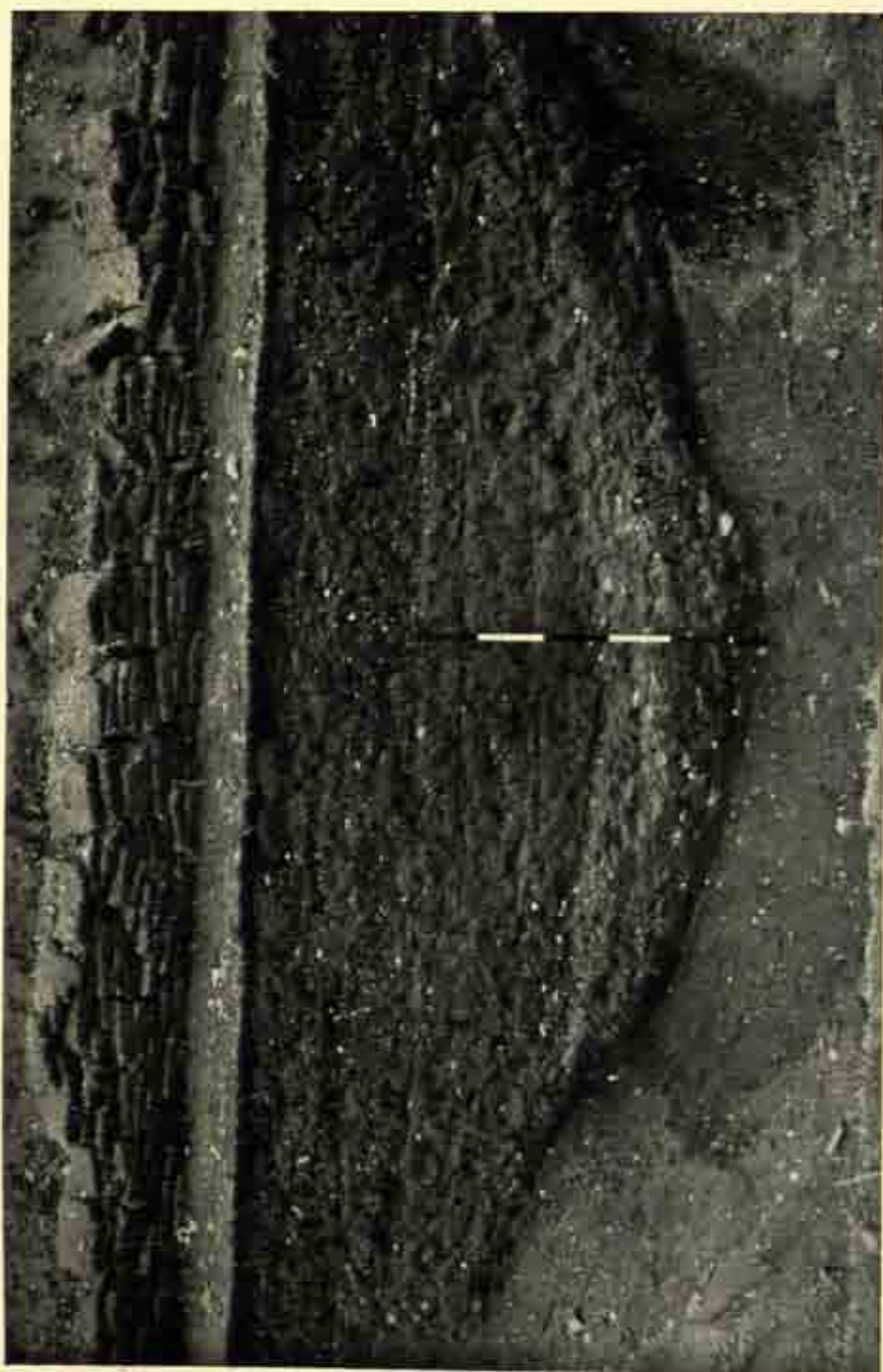


# PLATE I



NORWICH 'WOODHENG'.  
 Collum air-photograph of the site at an early stage of the excavations. The white lines were painted in line on the ground to mark the limits of luxuriant grass growth, indicating the presence of ditches. The scale (near the centre of the plate) is marked off in units of a yard each. (See p. 466).  
 Aerial, Mr. H. F. Low, Norwich Aero Club, 1935

PLATE II



NORWICH \* WOODHENGES  
Central portion of the inner ditch cleared to the undisturbed gravel (See p. 466)  
P. HALLAM ASHLEY



# PLATE III



NORWICH 'WOODHENGE'.

Central area skinned to the gravel surface. The white pegs show the positions of the eight post-holes and are placed in the same area as the ramp (See p. 467).

Prof. Hutton Ashby

PLATE IV



NORWICH "WOODHENGES"  
A Post-hole sectioned at its maximum diameter (See p. 468)  
Ph. Hallam Ashley



## Notes and News

### THE NORWICH 'WOODHENGE' (PLATES I-IV)

The September number of *ANTIQUITY*, 1929, opened with the Editor's statement that 'another Woodhenge has been found, just outside the City of Norwich', consisting of 'two concentric rings surrounding a circle of nine dark spots representing without doubt wooden post-holes'. The Editor went on to ascribe the monument to Beaker folk, probably from the mouths of the Rhine, where, as he remarked, timber circles abounded at that period.

The excavations, which I had the honour to direct on behalf of the Norfolk Research Committee, with the able assistance of Mr Rainbird Clarke, Mr C. W. Phillips, F.S.A., and Miss G. M. White, during August and early September of the present year, have confirmed in almost every particular the prognostications of the Editor. The fact that it is becoming increasingly possible to prophesy what the spade will ultimately reveal illustrates the advance of prehistoric archaeology towards the standing of an exact science. In the romantic stage of archaeology the attraction of the Unknown (including Treasure of Untold Value) was perhaps the chief stimulus that drove men to dig. In these more sophisticated days we undergo the strains and expenses entailed by modern standards of excavation to demonstrate objectively, what our knowledge of the already vast comparative material has led us to expect. The spade, like the surgeon's knife, serves increasingly to confirm diagnosis. The days when monuments and victims were cut up for curiosity are (to those who have faith) past and finished. We are entitled to hope, under the new Act, that the day is also finished for planting a giant electric pylon<sup>1</sup> upon a monument of the importance of the Norwich 'Woodhenge', within a few months of its discovery being announced to the world!

When the excavations began this year the two rings, the outer broken by an old field drain and the modern hedge, and the inner

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<sup>1</sup> I must add that the Norwich representatives of the Electricity Board extended facilities for utilizing their pylon for photographs and generally showed a keen interest in what was going on. No doubt, if representations had been made from the right quarter, the Electricity Board would have planted its pylon elsewhere.

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interrupted by what was evidently a causeway, were clearly visible on the ground, though no signs of post-holes or other features could be detected in the central area. In order to plan the existing features, which a continuance of the drought might have rendered indistinct or even obliterated, the areas of luxuriant vegetation were demarcated on the ground by a four-inch line of lime. An air-photograph was then taken by Mr H. F. Low of the Norfolk and Norwich Aero Club (PLATE I) which illustrates that our ground work was substantially accurate, apart from a gap immediately below the patch of nettles\* which prevented our marking the ground for part of the inner circle and put us slightly out of line in its neighbourhood. A five-yard scale was painted on the ground in the central area and is visible in the photograph. The pylon, of which mention has been made, stands outside the inner circle but within the outer one; the lines crossing the photograph obliquely are the actual cables.

The first step in the excavation was the cutting of an 11 ft. section from outside the monument to the edge of the central area within the inner ring, a distance of 100 feet. The beginning of this cutting, and the outline of the first rectangle to be moved from the central area, can be seen in the air-photograph. The outer circle proved to be a shallow ditch 12 ft. wide at the lip and 4 ft. 8 ins. deep below modern surface; the inner circle on the other hand proved to be a substantial ditch 28 ft. across and 7 ft. 8 ins. deep. Between the two ditches, both of which were shallow relatively to their width, were the remains of a bank; this was much ploughed down, but its width and the relatively small capacity of the ditches suggest that it can never have amounted to very much.

The outer ditch produced some 3rd century Romano-British sherds above a sterile layer overlying the primary silting; the rest of the ditch had been filled by material ploughed in during historic times. The inner ditch produced stratigraphical evidence of importance. Our photograph shows the central portion of the ditch cleared out down to undisturbed gravel (PLATE II). Over three feet of the infilling has been ploughed in at a time when chalk was used to marl the ground. Below this down to the bottom of the black zone of the ranging pole occurred quantities of Early Iron Age pottery, which has yet to be examined, and a strong admixture of Romano-British material, sherds and coins, which appear to date from the 3rd century A.D. It is evident that the

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\* These were caused by the outflow from a modern drain.



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Early Iron Age levels were disturbed in Romano-British times, an interpretation confirmed by the cutting of the inner ditch at the entrance where similar Early Iron Age wares were found with little or no Romano-British material. Below the Early Iron Age level there occurred a sterile zone of primary infilling resting on a thin charcoal stratum on the floor of the ditch. This charcoal seam, which is visible on the right of the section shown in PLATE II, thickened to six or seven inches at the deepest part of the ditch and disappeared on the left of the ranging pole. It produced 16 sherds of pottery and three or four 'crumbs' all of the same ware, resembling beaker on the inside and decorated in rusticated finger-nail style<sup>3</sup> on the outer surface.

The central area was stripped to the gravel and sand surface in order to recover constructional features. The sites of the post-holes, revealed by Wing-Commander Insall's original air-photographs, were found quite easily on removing the turf and topsoil, but they declined in number from nine to eight; this, in fact, was one of the few rectifications achieved by the excavation—the timber part of the monument was of horse-shoe plan, being open to the causeway entrance to the central area. In plan, therefore, the timber uprights resembled the arrangement of the inner horseshoe of bluestones and of the trilithons themselves at Stonehenge. No other traces of disturbance within the central area were met with, nor were any traces of hearths or other indications of occupation discovered. As might have been expected each of the post-holes was provided with a ramp; slight indications of them were to be seen in plan and they were in each case verified by sections. An interesting fact which emerged was that the ramps all point in the same general direction . . . up-hill towards higher ground, and away from the sudden slope down to the river Tas. In the photograph taken from the pylon and reproduced on PLATE III the axes of the ramps are indicated by white pegs. The significance of this fact will be obvious. The heavy oak trunks, which we found to have been three feet in diameter, were dragged downhill; but clearly they were not hauled across the very substantial inner ditch, and on the other hand the entrance to the central area is at right angles to this line and to the axes of the ramps; therefore the timber uprights were erected before the construction of the inner ditch.

Time and resources did not allow of the complete excavation of the monument, but the nature of the post-holes was satisfactorily

<sup>3</sup> Whether this was really done by the finger-nail or by an implement giving a similar impression I should hardly like to say.



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established from the excavation of two examples ; one of these was cut to reveal the post-hole and ramp in section, the other to obtain as perfect a section of the post-hole itself as could be obtained. The ramp of the first post-hole investigated sloped down obliquely to the bottom of the post-hole (7 ft. 10 ins. in undisturbed gravel) from a point 12½ ft. from its centre. A photograph of the second excavated post-hole at its maximum diameter is reproduced on PLATE IV.

The line of the original excavation was easy to distinguish in the field and can be seen particularly well towards the bottom right-hand corner of the plate. The other outer limit is visible approximately six inches to the left of the ranging pole. The outline of the actual post itself was indicated even more clearly and gives little sign of distortion. Naturally the wood itself has gone long ago, but the charred base was almost intact and sufficient traces of the charred sides have remained to preserve the outline of a trunk three feet in diameter. Both the posts examined were oak. Nothing of archaeological interest was found during the excavation of either post-hole.

The excavation of the central area was extended to include the entrance, which proved to be a causeway of firm gravel showing no signs of disturbance. One end of the inner ditch was cleared out, but the slope was so gradual that we got no primary material. A stratum containing Early Iron Age pottery was found overlying a layer of primary silting.

The horseshoe of timber uprights, having been shown to antedate the inner ditch, must be at least as old as the pottery obtained from the floor of that ditch in the first cutting. But it is obvious that the inner ditch was dug round the timber construction as part of one scheme, since the causewayed entrance is placed symmetrically thereto. Therefore for all practical purposes the pottery from the floor of the inner ditch can safely be used to date the monument. This is not the time or the place to discuss the chronology of this type of pottery in detail, but it is sufficient to say that it does not differ substantially in age from the familiar Beaker. Exactly similar pottery is illustrated by Curwen from Whitehawk Camp, Brighton, where it was found in association with Beaker, and separated by a phase of natural silting from the Neolithic A2 pottery normal to the site.<sup>4</sup> The pottery is common to southeast Britain and the Low Countries, and probably reached us as one of the many elements which came from the Lower Rhine area during the Beaker Period.

<sup>4</sup> *Antiquaries Journal*, 1934, p. 119, fig. 57.



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The Norwich 'Woodhenge'—I use the term generically—like the original Woodhenge and like Stonehenge itself, seems to have been primarily a sacred place, in the proximity of which are the remains of numerous round barrows. As larger areas are searched from the air it may be expected that many more monuments of this kind will be revealed. For permission to excavate the Norwich site and for other assistance we are greatly indebted to the owner of the land, Mr Russell Colman, Lord-Lieutenant of the County. J. G. D. CLARK.

### THE WHITE HORSE OF KENT

The reproduction by Dr R. E. M. Wheeler in his *London and the Saxons* (London Museum Catalogues, no. 6, 1935) of a wood-cut from Richard Verstegan's *A Restitution of Decayed Intelligence in Antiquities* (Amsterdam 1605, London 1634) depicting 'the arrivall of the first ancestors of Englishmen out of Germany into Brittain' (see FIGURE), is a timely reminder that Richard is the earliest authority to attribute the badge of a White Horse to the leader of the Saxon invaders.

How far Richard was correct in saying that 'a horse argent rampant in a field gules' was the ancient device of Saxony, and that 'they have there long since for many ages together borne', we cannot tell. He had, while at Christ Church, distinguished himself as a scholar of Early English history and the Anglo-Saxon language, and it seems likely that when as a consequence of his religious beliefs he migrated to Amsterdam, he had the necessary facilities for an investigation of such matters had he wished to continue his studies. In any case there seems nothing inherently improbable in his statement, since the Teutonic predilection for horse-mythology is well known, and the White Horse was certainly adopted in later years as a device both by the State of Saxony and the House of Brunswick. Whatever basis of truth there may or may not be in Richard's illustration of Hengist's landing in Kent, the fact remains that John Speed, writing his *Theatre of the Empire of Great Britain* only five years later in 1611, boldly assigned a device of arms to each kingdom of the Heptarchy, and to the kingdom of Kent in particular he assigned the white horse on a red ground, saying furthermore that it was the emblem of the Saxons.

In criticism of Richard's illustration, it may be pointed out by students of heraldry, and with some justice, that armorial bearings of this sort were scarcely known until the later part of the 12th century, and further, that if there were certain devices and figures in use in

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earlier days (as the Bayeux Tapestry seems to indicate), then they can have had no specific relationship to the later system of heraldic bearings.

By the end of the 18th century, the customary use of the White Horse emblem, reinforced perhaps by a Royal Warrant of 1751 which ordered the Hanoverian Horse to be worn by certain ranks in the 50th (Royal West Kent) Regiment, had become well recognized; and Hasted,



HENGIST AND HORSA LANDING IN KENT, A.D. 449

in his *History of Kent* (1797 ed., I, 64) commenting on Verstegan's statement, adds that 'similar to which are the present arms of this county the only difference being the colour of the field'. A further 18th century use is well attested by the Kentish tradesmen's tokens. During the next century and a quarter the wonted right of the County to bear the device of the White Horse was never seriously questioned,



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and in the 19th century, as Mr A. J. Golding has pointed out to me, the rampant horse was used freely as a local trade-mark, as the badge of the County Cricket Eleven and the Kent County Constabulary, and as the distinguishing mark on Kentish hop-pockets.

This respectable ascription of the White Horse however, is sometimes overlooked in favour of a rather tenuous belief that the standard of the Saxons, a banner emblazoned with a rampant White Horse, was set up on the Kentish Standard Stone or White Horse Stone by the two chieftains Hengist and Horsa at the time of the battle of Aylesford in 455. A variation of the legend says that the Saxon standard was found under the stone after the battle.<sup>1</sup> In this connexion the present White Horse Stone at Aylesford, a standing sarsen which may be a relic of a megalithic burial-chamber, can be dismissed from consideration at once: it received its name in the early nineteen hundreds from a local antiquarian.<sup>2</sup> The original or Lower White Horse Stone known also as the Kentish Standard Stone, has now disappeared, but it stood formerly at the crossing of the Pilgrim's Way with the Rochester-Maidstone road at Aylesford, and its name, though a little older than that of the present White Horse Stone, does not appear earlier than 1834. The stone itself was quite probably part of a ruined megalith.

It would also be very easy to show that the claims of two other suggestions for the origin of the White Horse—namely, the familiar horse-and-chariot coins of the Early Iron Age and the various items in the horse complex of the Early Iron Age, cannot now be seriously maintained. In fact, all things considered, it seems very unlikely that the White Horse of Kent existed before the early part of the 17th century.

R. F. JESSUP.

## RAG-WELLS

Miss M. D. HILEY sends us particulars of a rag-well, known as St. Bennet's Well.

<sup>1</sup> It is near the high-water mark on the shores of Moray Firth about 1½ miles SSE of Cromarty (6 inch map, sheet LXVII) and is situated at the foot of a beautiful little glen which runs inland from the coast.

<sup>1</sup> But it should be said in fairness to Lamprey, the supposed author, that he himself recognized the thinness of the White Horse standard tradition. The 'Standard Field' of Stukeley, *Itinerarium Curiosum*, 2nd edition, II, plate XXXIII, has obviously no connexion with it.

<sup>2</sup> It seems possible that the name may have been suggested by a hypothetical earlier name 'hoar stone', but I know of no specific evidence for such a name here. O.G.S.C.

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' In order to insure the fulfilment of the wish it is essential that the wisher should drink the water and leave something of his personal attire. When the writer visited the spot there was a heterogeneous collection of " rags " hanging on the branches.

' Mr Francis Scott tells me that the site is locally supposed to be the place of judgment. It is close to the ruins of St. Bennet's chapel and the ground is said to be cursed as it was stolen from the Church. Even at the present day the owner has to provide each year at Christmas-tide 8 cwt. of oatmeal free for the poor of the parish. This has been operative since 1630 and though one owner tested the matter in the highest court of law in Scotland his appeal was not allowed '.

### A ROMAN VILLA AT DITCHLEY, OXON. (PLATES V-VI)

The existence of a Roman building in Watts Wells Field South, on Lodge Farm, Ditchley, has long been known to archaeologists. About 60 years ago Pitt-Rivers cut trial trenches in which he uncovered part of a pavement of *opus signinum*. Last year Mr E. J. Walford, of Coventry, flying over the site noted the remarkable completeness of the plan outlined in the growing crop. A series of photographs taken by Major G. W. G. Allen revealed features of considerable interest which promised to throw light on several problems connected with the villa-system. Aided by the support of the Ashmolean Museum an Excavation Committee was formed with Professor R. G. Collingwood as Chairman. Mr Ronald Tree, M.P., the owner of the site, agreed to serve on the Committee, to which he granted every facility for the exploration. With consent of the tenant, Mr C. Hunt, and H.M. Office of Works work began 26 August. The remains have been surveyed by Mr J. J. Leeming. Work was also carried out by Mr D. B. Harden on the neighbouring section of Grim's Ditch, of which the relation to the Villas in the neighbourhood has already been discussed by Mr O. G. S. Crawford (ANTIQUITY, 1930, IV, 303-15, with map, p. 305).

The site lies about 430 ft. above sea-level on the north side of a small valley, a tributary of the Glyme. The ground occupied slopes gently to the south and east. The air-photographs show a large courtyard enclosed by a wall and ditch. The entrance on the south side is approached by a road, flanked by ditches and leading in the direction of the Akeman Street, 2 miles distant. At the upper end of the courtyard lies the house, facing south. It consists of a central block with



PLATE V



ROMAN VILLA, DITCHLEY: VIEW FROM SOUTHWEST, MAY 1934. FIELD UNDER CORN (500 ft. 472)  
Aerial, Major G. W. G. Allen

PLATE VI



ROMAN VILLA, DITCHLEY: VIEW FROM SOUTHEAST, MAY 1934. FIELD UNDER CORN (See p. 471)

The line crossing the ditch in the foreground is a modern field drain

*At a pk. Major G. W. C. Allen*



PLATE VII



THE WARRIOR OF CAPESTRANO, AQUILA (See p. 477)

*By courtesy of Prof. G. Moretti, Director National Museum, Rome*

PLATE VIII



THE WARRIOR OF CAPESTRANO, AQUILA (See p. 477)  
*By courtesy of Prof. G. Moratti, Director National Museum, Rome*

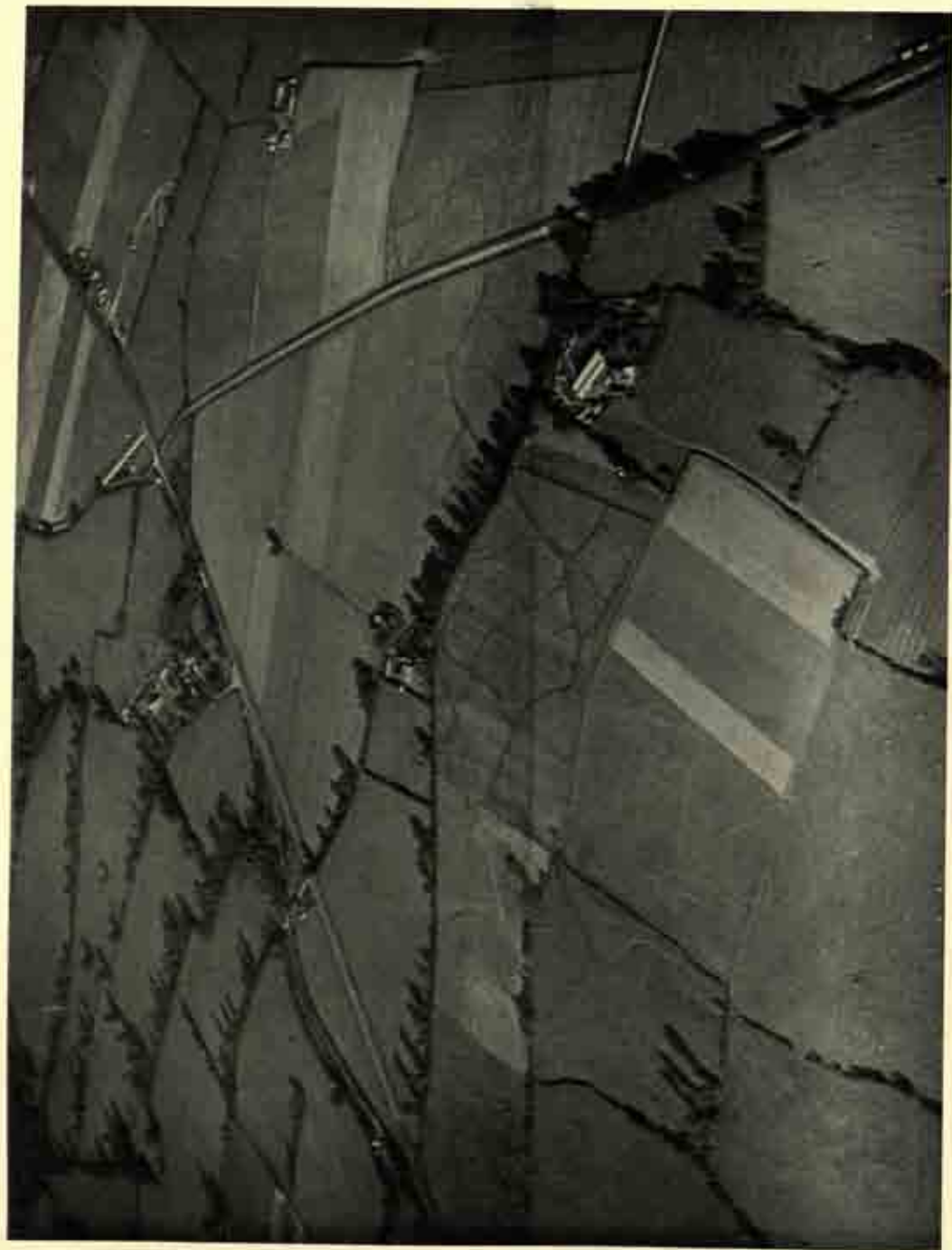


PLATE IX



BLACK DITCH FIELD, STANTON HARCOURT (see p. 476)  
Aer. ph. Major G. W. G. Allen

PLATE X



TRINDAL'S FARM, CHILWORTH; showing unusual markings not found elsewhere (See p. 479)  
Aeropht. Major G. W. G. Allen



PLATE XI



TRINDAL'S FARM, CHILWORTH: showing unusual markings not found elsewhere. (See p. 479)  
*Air-phot. Major G. W. G. Allen*

PLATE XII



ESTHONIAN PEASANT LIGHTING HIS PIPE WITH FLINT AND STEEL. (See p. 479)  
*Fr. De T. Manninen, 1925 (by courtesy of the Estonian Museum)*



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projecting wings at each end. At the back is a range of small rooms, and a verandah extends along the whole of the front and sides. The parched appearance of the crop suggests pavements at the northwest corner. In front of the house is a dark circle now known to represent a well. Extensive but incomplete lines of masonry indicate further buildings at the southern end of the courtyard, while a small incomplete circle appears near the centre of the west side.

During the excavations the house has been completely cleared. The southern building and the courtyard, which have been much destroyed by the plough, have been explored by a series of trial trenches, as a complete exposure of these remains did not seem to justify the expenditure involved. The work has provided satisfactory evidence for the history of the site and of the various building periods. At present only a tentative chronology can be put forward as the detailed examination of the pottery is not yet completed.

The earliest building on the site of the house is a rectangular structure of timber, measuring 55 ft. by 55 ft. The post-holes, about 9 inches in diameter, discovered under the western wing and the central block of the later house indicate a building of slight construction which must have been destroyed when the earliest masonry was laid. The second house consisted of a central block with projecting wings at each end, but no verandah. The inconvenience of a sloping site was avoided by a massive substructure of masonry raising all the floors to approximately the same level. Above this the building was half-timbered with rubble-filling between the beams and a roof of tiles. The lower storey had wooden floors carried on joists beneath which were voids varying from a few inches to nearly 3 ft. in depth. The entrance was in the eastern wing, where a flight of three stone steps (indicated on the air-photograph by a white blur) covers the whole of the front and returns along the eastern side. At the other end of the central block the long narrow division (3) contained the stairs leading to the upper storey. It is clear that the earlier building was not long in use and it may tentatively be assigned to the period A.D. 70-80 when the site was first occupied. The deposits contemporary with the stone substructure contain nothing that need be later than A.D. 100, and in the forecourt a coin of Trajan in mint condition was found in a position suggesting that it had been dropped during the building operations.

During the second century a long corridor was added at the back of the house. It had a floor of stones and gravel following the slope of the ground, and was roofed with Stonesfield slates. Later partitions



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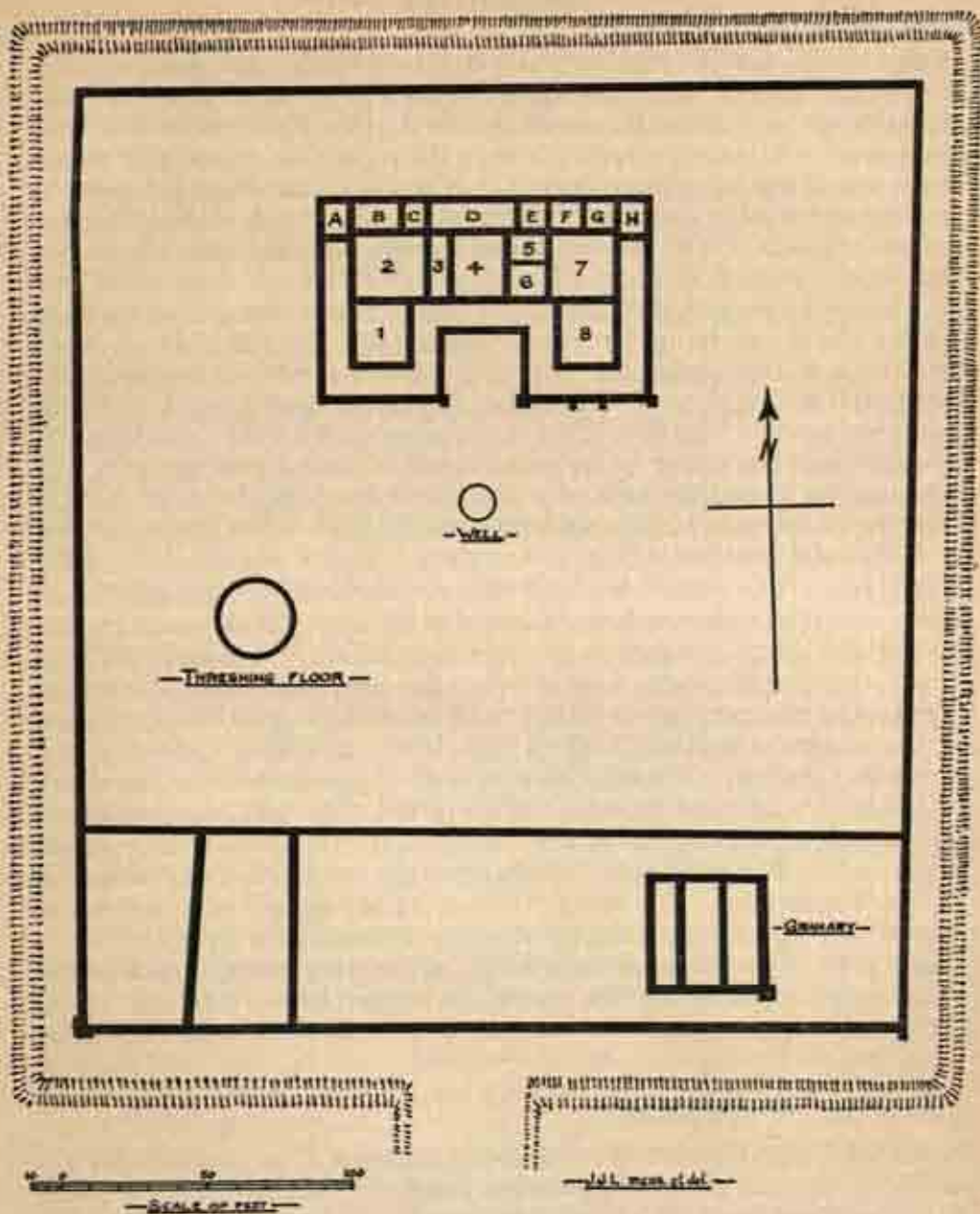
were inserted at the east end, carrying wooden floors similar to those of the main house but at a lower level. This enlarged house was partially burnt. The fire seems to have been most violent in the west wing and at the back, where thick deposits of burnt debris were uncovered lying where they fell. Elsewhere the evidence suggests that flaming timbers from the roof fell against the outer face of the masonry substructure and were consumed, burning the face of the stone to a bright red.

Large piles of the rubble-filling of the half-timbered work lying against the masonry suggest a period of desertion and gradual decay (cf. the southern building *infra*), a hypothesis which is corroborated by the almost complete absence of coins and other objects of the third century.

The last house was on a grander scale. The original substructure was used as a foundation for a two-storeyed building. Along the front and sides a verandah of a single storey was added, the parts at the rear of each wing being partitioned off. The series of small rooms on the north side, was rebuilt on the older foundations. The entrance remained in the eastern wing, where the new verandah was adorned with a colonnade. The four projecting piers of masonry carried stone columns, 1 foot 6 inches in diameter. Considerable parts of two bases and one complete drum have been recovered. The masonry of this period was poor in comparison with the earlier work. Many weathered and perished stones were used, the defects being disguised by the thick coating of plaster with which the outer walls were finished. Pavements of *opus signinum*, laid on thick beds of obliquely pitched stones which in turn rested on a filling of clay and rubbish, were found in the four rooms in the northwest part of the house. Elsewhere the plough had removed the surface but the bedding survived in most of the rooms. This final rebuilding should probably be attributed to the Constantinian period though the evidence for this date is admittedly slight. The series of coins shows a marked predominance of issues later than A.D. 364 and proves that the site was occupied up to the end of the fourth century and probably later. The house gradually fell into decay. Squatters or travellers camped in some of the rooms where their fires burnt holes in the pavements, leaving a thick layer of wood ash at a period when part of the roof had yet to fall.

The sequence at the southern end of the courtyard confirms the evidence of the house. At the lowest level were traces of a wooden structure with sleeper beams and a clay floor. Above this an extensive





ROMAN VILLA, DITCHLEY: PLAN WITH WALLS AND DITCHES RESTORED

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building or series of buildings occupied the whole end of the enclosure. The stone base of the walls was robbed to below floor-level over the greater part of the area, but in those places where the remains had been preserved to a greater depth it was evident that the upper part of the walls was of timber plastered with clay, and that they had been allowed to decay gradually, covering the occupation-level with a thick layer of yellow material. The latest pottery found under this dates from the end of the second century. The evidence of human occupation and the size of the building lead to the conclusion that it was used as barracks for the slaves cultivating the estates of the villa. After an interval, when the site was reoccupied, the barracks were not rebuilt, but near the southeastern angle of the courtyard a granary was erected over the fallen material. This is probably contemporary with the Constantinian rebuilding of the house. The small circle of masonry on the west side represents a threshing-floor of a primitive type, beneath which lay the remains of an earlier circle evidently unknown to the builders.

The ditch surrounding the courtyard was v-shaped and nearly 6 ft. deep. The lowest levels of the silt contained pottery of Flavian date, after which the ditch was allowed to fill up. Its existence implies an original bank surmounted by a timber stockade. This survived until the final rebuilding of the house when the bank was used as a quarry for the material required to fill the voids below floor-level. The masonry of the courtyard-wall also suggests that it was erected at a late date and resembles some of the contemporary work in the house.

The sections cut through Grim's Ditch show that the bank rests on plough-land and agree with the dating suggested in the article referred to. The evidence of a change in the methods of cultivation in the latest period and the high proportion of late coins found on the site suggest that the villa-system, though not necessarily the house, survived many years after the beginning of the 5th century and that it was still working when the Ditch was erected to bar the roads leading from the north and east.

C. A. RALEGH RADFORD.

## TEMPLE OF ARMANT

Mr OLIVER H. MYERS writes that the Egypt Exploration Society is trying to reconstruct graphically the temple of Armant and the scenes which covered its walls. The temple was destroyed during the latter half of the 19th century to make the foundations of a sugar factory. In 1857 Francis Frith took at least three photographs of this temple,



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which were published in *Egypt and Palestine*, 1858-9, and *Egypt, Nubia and Ethiopia*, 1862. Maxime du Camp published another in *Egypte, Nubie, Palestine et Syrie*, 1852, and Félix Teynard one in *Egypt et Nubie*, 1858. The negatives of these, or any other prints or negatives of this temple, would be of the greatest assistance, and the Society (200 Euston road, London, N.W. 1) would be extremely grateful to anyone who could help in the search for them.

The temple was built by Cleopatra and was the only one standing at Armant in recent times. Armant is variously spelt Armant (correct), Erment, Ermant and Hermonthis.

### THE CAPESTRANO WARRIOR (PLATES VII-VIII)

We are very much indebted to Professor G. MORETTI, Director of the National Museum at Rome, for allowing us to publish the following note and illustrations, in response to our request. Dr RANDALL MACIVER, who has translated the text, tells us that he is in entire agreement with Professor Moretti's views.

The territory of the Vestini, extending along the slope of the Gran Sasso towards the Adriatic, has never been explored, and even chance finds had provided us with little evidence of its life in the so-called 'Italic period' until almost the other day. Last winter however there was found at Capestrano, in the province of Aquila, near the source of the little river Tirino, a tributary of the Atirno, which rises in a cleft of the Apennines between La Majella and the Gran Sasso, a statue now known as the 'Warrior of Capestrano'. This has no equal in Italic art for rarity and importance.

The figure is of life-size, 1.70m high, and stands rigidly erect on a rough plinth between two shafts which serve as supports. It is naked except for a full complement of armour and weapons. The armour consists of helmet, mask, cuirass formed of a single disk at front and back, and stomach-piece. The weapons are a sword and dagger worn in front of the figure, and a spear resting against the outside of each of the stone supports.

Down the length of the right-hand support runs an inscription of which the epigraphy and linguistic character are consonant with the archaeological style of the statue and its stylistic peculiarities. For while the conception and arrangement of the figure place it in the 6th century before Christ, all the details of arms and equipment can no less

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exactly be paralleled by finds of the same period made in tombs of the Early Iron Age in the central regions of Italy. The wide-brimmed helmet and the mask are eminently Picene; the sword and dagger have their equivalents in Etruria, especially in respect of the hilts decorated with human and animal figures; while the disc-cuirass, girdle, torque and armlets can be matched from almost any of the cemeteries in Samnium, such as Aufidena, or in the territory of the Marsi, Marrucini, Peligni, Picentini.

The statue of Capestrano, severely stylized like every work of that period, shows us the true and proper figure of a man in the attributes of his most usual profession, that of a warrior. The absence of clothing, the monumental character of the headgear, the hierarchical gesture of a symbolical axe laid against the left shoulder—all these suggest something between human and superhuman. We may therefore regard it as a semi-divinized warrior (*guerriero eroizzato*).

At a short distance from the torso (for the legs and base of the statue were found subsequently, as a result of explorations made by the Service of Antiquities), was found the bust of a woman. The head was missing but there were new and important details in the clothing of the figure. It is possible that the woman, whose proportions were smaller, viz. about two-thirds of life size, was some sort of relation or dependant of the warrior. No tomb however could be identified as belonging to either of the two figures, though 21 tombs were opened in the course of trial excavations made in the hope of determining the archaeological background of this unexpected and important discovery.

### NEW AIR-PHOTOGRAPHS (PLATES IX-XI)

The first of these air-photographs (all of them taken by Major Allen) is of Blackditch Field, Stanton Harcourt (Oxon 32 SW, SE, 38 NW, NE), and reveals markings of three distinct periods (PLATE IX). Certainly the oldest are the parallel black lines leading to two enclosures in the middle of the picture. This may be prehistoric or Romano-British. Next come the remains of the medieval field-system, represented by the parallel ridges in the foreground. It is interesting to note that the dark furrow-lines between the ridges are continued by those dividing the existing allotment-strips in the triangular space on the right, next to the road; also that this intensive and prolonged medieval cultivation has not obliterated the earlier remains. (For a similar phenomenon in the same neighbourhood see ANTIQUITY, 1933,



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vii, 293). Finally we have the modern field-system which conforms partly but not wholly with that which preceded it.

The other two air-photographs (PLATES X-XI) give two views of the same site, Trindal's Farm, Chilworth (Oxon 40 NE). No explanation is offered, however. Nothing quite like the markings has been encountered before elsewhere, and since surface observation is unlikely to help, excavation seems indicated. They are probably medieval. The geological formation is gault clay, overlaid by plateau gravel.

### PRIMITIVE FIRE-MAKING (PLATE XII)

The accompanying illustration of an Esthonian peasant living in the parish of Halliste in the arrondissement of Pärnu, and photographed in 1925, is not without interest to those who follow the survival of ancient methods in modern times. This Esthonian is engaged in lighting his pipe with a 'briquet' (flint and steel) and the very idea that such primitive methods still survived as late as 1925 is apt to raise a smile. But up to the year 1835, every house in England possessed and used its tinder boxes and the unlucky individual who had to rise betimes would spend from three minutes to half an hour in securing a light, unless the tinder was damp, when the process was still further prolonged.

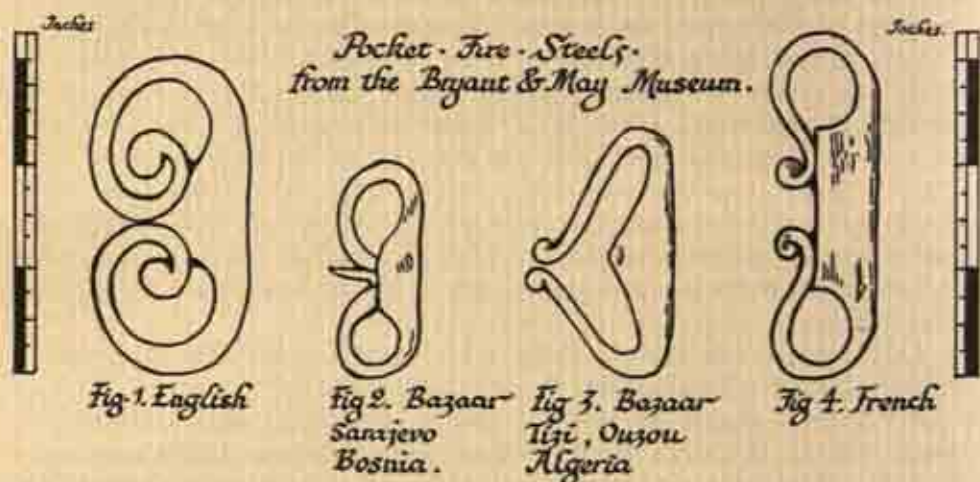
How many children today, who have read Hans Andersen's 'Soldier and the Tinder Box', have ever actually seen one? Yet its ancestry comes down to us from pre-Roman days, unaltered by the passage of time, till, with the coming of Queen Victoria, friction matches swept the time-honoured tinderbox aside, so that it only lingered on in little backwaters of Europe or among savage tribes. Familiarity breeds contempt and the ease of the friction-match has made us forget that fire-making is man's greatest achievement, and that the tinder-box has its place in the study of cultural anthropology.

The essentials of fire-making are flint or flinty stone such as chert, chalcedony, jasper or agate, having a sharp cutting-edge, and a piece of suitably tempered iron or steel. The flint should be struck sharply and at an angle upon the steel so as to shave off a tiny fragment of the metal, heated to white heat by the percussion and glowing as it falls upon the tinder. And what is tinder? It may be charred rag, or 'touchwood' or a dried fungus steeped in saltpetre, generally known as 'amadou' or German tinder. The best kind of flint for this purpose is from the chalk of northwest Europe and notably from Brandon

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in Suffolk, where the industry of flint-knapping still survives and whence 'briquet' flints were exported by Fred Snare to Spain, South America and Borneo as late as 1924.\* Brandon flints have also been found in Syria and Siberia. Yet another source of supply is the Kentish flint shipped as ballast to Shanghai, where it is used partly for china making and also for strike-a-lights. In the southeast of Europe a different type of flint is used. Not infrequently these are enclosed in leaden sheaths for protection. Turkey is the chief source of supply.

The steel held by the Esthonian peasant is of a familiar type, which has a wide distribution. It has two tangs which form a hand-hold at the back. It appears to be quite without ornament, though it



is possible that the tangs may be twisted into a spiral or knobs. In general form it might be English. Indeed in the Bryant and May Collection (no. 265) is an almost exact counterpart, described as English. Other similar forms in the same collection are no. 177, bought in the bazaar at Sarajevo in 1895, no. 182 from the bazaar in Tizi Ouzou, Algeria (1898) and possibly no. 329, a French example (see FIG. above).

One thing is wanting in the photograph, and that is the pouch or box in which the flint and steel were carried. These receptacles display a great variety of material. All are small so that they may conveniently be carried in the pocket. European tinder-pouches are usually about

\* See ANTIQUITY, March 1935, pp. 38-56, 8 plates.





PLATE XIII



THE PORT OF WAL-WAL, ADYSSSIOLE (See p. 481)  
In its ventral features the tort closely resembles many polychaetes, found in Britain  
Ph. Plancher Noyes



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three inches square, the opening at the top being closed by a flap. Leather is the material most commonly selected, though tapestry, woolwork or beadwork are also used. Flint and tinder are carried inside, but the steel is very often fixed to the bottom of the bag. The sulphur-tipped match of either paper or wood, which figures in the outfit of the domestic tinder-box, was not usually carried with the tinder-pouch, which was mainly used for lighting the pipe direct from the tinder without the intervention of the match. FRANK STEVENS.

### THE FORT AT WAL WAL (PLATE XIII)

Wal Wal is a fort built by the Italians in Abyssinia at some date subsequent to 1928. It is actually on the edge of a small ravine. The purpose for which it was built was to command the wells which lie in the small ravine just below, beyond the picture on the upper side. There are some 60 to 100 wells with narrow shafts of between 60-100 ft. depth through sandstone. The picture is taken looking towards the ravine, and the tracks on the upper portion of it represent the motor road to Warder.

It will be seen at a glance that the fort very closely resembles in all its essential features many prehistoric hill-forts in Britain. It consists of a ditch and bank with a wooden palisade planted along the foot of the bank on the inside. There is one entrance defended by a subsidiary bank thrown out, exactly as at Hod Hill and Hambledon Hill (see *Wessex from the Air*, plates 1 to 3).

The mushroom-like objects in the interior are native Somali huts (gurgi). They are portable, are made by women, and are roofed with branches on which camel-mats (herios) of plaited grass (also made by the women) are placed. The roofing consists of branches bent over to meet in the middle, where they are lashed together with thongs. A not dissimilar method was employed for reed huts in ancient Mesopotamia: in one instance however the ends of the reeds were left projecting beyond the roof on either side. (See Mallowan in *Iraq*, April 1935, II, 31-2, fig. 19). (These herios are also used on the backs of camels where they are placed three deep in order to support the load, and for beds laid on the ground). It should be mentioned that these very thick roof-coverings maintain an equable temperature against extremes both of heat and cold. These huts obviously house native troops.

The two huts at the right hand side of the enclosure are the headquarters where the officers live and work.

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It will be noticed that at one end of the upper building is a dead tree, to which a post has been attached from which the Italian flag may be seen flying.

The (H.Q.) buildings in question are called 'arish' in Somali. The sides are made of wattle and daub and the roof of grass or mud.

The two excrescences are puzzling but probably represent machine-gun posts. It will be noticed they are approached by a communication trench and that actually two men are to be identified in the right hand post by means of their white turbans. It is presumed that the machine gun would be placed on a tripod on the central platform, and that if thus raised it would just clear the surrounding parapet.

Finally, the whole is surrounded by an artificial zareba of thorn which can be seen in the foreground and in the top corners as a tangle of light-coloured brushwood. There is an entrance to the zareba in the right hand bottom corner, where two figures may be seen, one standing and the other squatting, in front of them being a bush that is doubtless drawn in to close the breach at night and whenever occasion may require it.

The whole is a very informative example of the way in which similar causes produce similar effects, both in the past and in the present.

The general plan of the fort has no doubt been designed in accordance with local native tradition, but the European control exercised in this instance is obviously responsible for such very modern features as the machine-gun posts.

We are indebted to one of our readers for drawing our attention to this very interesting example of the past and the present.

## PREHISTORIC CONGRESS

The Second International Congress of Prehistoric and Protohistoric Sciences, following on the First Session held in London in 1932, is to take place at Oslo in August 1936.

Professor J. L. Myres is one of the General Secretaries of the Congress and the National Secretaries for Great Britain are Professor V. Gordon Childe (The University, Edinburgh) and Mr C. F. C. Hawkes (British Museum). Copies of pamphlets giving general particulars of the Congress (with form of enrolment) may be obtained from the Bureau at the UNIVERSITETETS OLDSAKSAMLING, OSLO, and either of the British Secretaries will be glad to answer, as far as they can, any further enquiries.



## Recent Events

*The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.*

Forthcoming excavations are hardly 'recent events', but cannot for that reason be ignored here; the fault is rather with the inadequate title. Dr Ernest Mackay left England in September for India, to carry out excavations on behalf of the American School of India and Iranian studies and the Boston Museum of Fine Arts, U.S.A.



The site selected is in Sind, at Chanhudaro, about 80 miles south of Mohenjo-daro. It lies on the eastern bank of the Indus, and is a large (and therefore once important) site. The upper levels appear to be slightly earlier than those of Mohenjo-daro. As the river is *now* some distance away, it is hoped to be able to reach the earliest strata and perhaps find remains of the Amri culture in the lower levels.



This is the first expedition to start work under the new regulations allowing outside bodies to excavate in India; and we feel sure that, if the Government of India is generous and helpful to the excavators, it will be by no means the last; indeed, if the foolish attitude of some other governments is maintained much longer, it may well follow that the activities of excavators will be deflected eastwards to India, to the loss of the aforesaid foolish ones.

*Irish is clearly not*



Mr W. B. Kennedy Shaw's explorations in the Libyan Desert were described by him in three articles in *The Times* (6-8 August). Some account of the archaeological discoveries will be published in a later number of *ANTIQUITY*.

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The image of a camel has been found for the first time in a palaeolithic camp at Kostenki, a village in Voronezh province. Prof. N. Y. Efimenko, the Soviet archaeologist, is excavating the site.

The camp was a settlement and was first unearthed in 1701. A dug-out, measuring 50 feet, presumably used for living, has been found adjacent to some big pits. Numerous sculptured objects made of soft stone and mammoth tusks have been revealed. Many of these represent women's figures and animals such as mammoths, cave lions, bears, and now a camel. (*Daily Telegraph*, 29 July).

o o o

The excavations which have been carried out by the joint expedition of the Museum of the University of Pennsylvania and the American School of Oriental Research at Tepe Gawra are of great importance. Mr Charles Bache, field director, in a report circulated by Science Service, Washington, D.C., states that he has now reached the level of the twelfth city (counting from above) on the site. This he dates at about 4000 B.C. Exploratory trenches have revealed the existence of eight earlier levels. In the recently discovered twelfth city, which appears to have been destroyed by fire, no trace of metal was found, and it is concluded that the inhabitants were still in the neolithic stage. Its architecture is superior to that of the three cities immediately following. (*Nature*, 27 July).

o o o

Dr J. R. Acherley, who has returned to Melbourne from New Guinea, reports the discovery in a limestone cave about 30 miles inland from Salamaua of a shelved mausoleum in which were large numbers of mummies apparently of great age. All the mummies were sitting with their chins on their hands and their elbows on their knees. (*The Times*, 18 July).

o o o

A site in the diatomaceous deposit of the Lower Bann Valley was excavated in June 1934 by the Harvard University Archaeological Expedition to Ireland. It lay at the base of an extensive deposit of diatomite where some thirty hearths were discovered. Implements of flint in addition to three polished stone axes were found. Typical



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of the industry are pointed flakes with a superficial tanging of the bulbar end. The site never served as a place of permanent habitation, since the hearths show evidence of seasonal floodings by the river. A nearly complete pot from quite near the site has proved to belong to the Windmill Hill family of Neolithic ware, according to Prof. V. G. Childe. The archaeological evidence points to about the beginning of the second millennium B.C. as a rough date for the culture. Such a view is substantiated by Prof. K. Jessen's palaeobotanical studies at the site, and on the basis of his work Late Atlantic to Early Sub-Boreal time is probable. As a whole, the industry seems to be an indigenous North Irish development derived from earlier coastal elements which had come in contact with a fully developed neolithic civilization. (*Nature*, 13 July; summary of a paper read by H. L. Movius before the Royal Irish Academy, 25 June).

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A remarkable find of a mosaic flooring has just been made by Professor G. H. Baxter behind the Mosque of Sultan Ahmed within the area occupied by the Palace of the Byzantine Emperors.

The mosaic, which appears to cover an area of at least 40 square yards, is believed to be contemporary with that uncovered last year by Mr Thomas Whittemore in the narthex of St. Sophia. The work is very delicate and the colouring excellent. The uncovering is being carried out under the personal direction of Professor Baxter, and that part of the floor which has already been cleaned is intact. (*The Times*, 19 July). Since this first announcement more of the mosaic has been disclosed and great results are expected from further excavation. Prof. Baxter has written a lengthy account of the present position for *The Times*, 26 and 28 October, illustrated.

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Excavations at the small hill-fort on Shenberrow Hill above Stanton, Gloucestershire are described by Mr J. M. de Navarro in the *Evesham Journal* (3 August). The pottery found pointed to an Iron Age date for the construction of the ramparts; and the finds made inside 'indicate that Shenberrow was no mere camp of refuge, but a site of settled habitation'.

'One thing is clear, that among the animal bones and teeth, those of the sheep formed a high percentage; the spindle-whorl shows that

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the inhabitants of the camp spun their own wool. One is tempted to think that the medieval sheep-breeding on the Cotswolds had a longer history behind it than heretofore imagined. Air-photography in the neighbourhood followed by excavation may prove the existence of ancient fields; whether the inhabitants of Shenbarrow Camp ground their own corn is not yet possible to say, but grind corn they did as fragments of quern stones, found during the excavations, show'.



Seven miles almost due south of Shenbarrow are the remains of the Notgrove long barrow. Here Mrs E. M. Clifford and her collaborators have been uncovering a most interesting complex of standing stones and dry stone walling. Near the middle of the long barrow is a circular structure of dry stone walling, called by the excavators 'the Rotunda', in which human bones were found. The exact interpretation of the remains found is by no means easy and must be left to those engaged on the spot.



Some well-preserved dry stone walling has been uncovered at an entrance to the Iron Age hill-fort of Maiden Castle, near Bickerton, Cheshire (not to be confused with Maiden Castle, Dorset, where too the prehistoric town walls have been uncovered this summer). The work has been carried out by the Rural Community Council (*Manchester Guardian*, 6 September; illus.).



The skeleton-silhouettes described and illustrated in *ANTIQUITY* 1933, VII, 470 (plates I, II) have been protected by a shelter erected over them. They can now be seen by any whose inclinations lead them to the province of Emst for holidays or study.



Remains of stone huts have been found at Engaruka, 118 miles west of Arusha, Tanganyika, beyond Lake Mangara. It is said that 'immense stone ramparts and fortifications' are to be seen, but we



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suspect merely remains of terrace-cultivation. Some 40 rock-engravings were also seen. We understand that further investigations are being made. (*East African Standard*, 2 August).

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The *Cornish Times* (6 September) asks for information as to the whereabouts of the Rillaton gold cup, which disappeared so mysteriously some years ago. The cup was found in 1837, near the Cheesewring, and passed eventually to Osborne House, Isle of Wight, where it was last heard of. It was exhibited at a meeting of the Royal Archaeological Institute, 7 June 1867. (See article by Edward Smirke, *Arch. Journ.* 1867, xxiv, 189-95; and Hencken, *Cornwall*, 1932, pp. 301 for other references). For the last thirty years the cup has been 'lost' and its hiding place remains unknown in spite of repeated search.

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Excavations at Jarlshof, Shetland, were continued last summer, and a large hut-circle, which has a remarkable underground chamber, was uncovered. The chamber is so small (barely 6 feet square and 2 ft. 8 ins. high) that its use as a living room seems hardly possible. Some four miles from Jarlshof, Dr Alex. O. Curle has investigated some ruins which prove to be a primitive iron furnace, with the dwellings of the workers and grave mounds of earth and stone. Some pottery found here was quite distinct from that at Jarlshof, being 'scored with deep grooves, forming chevrons and lozenge patterns'. From report by Dr Curle in *The Scotsman*, and *The Glasgow Herald*, 6 September.

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An account of the ruins of Quiriguà, the site of a great Maya civilization, and of the work of the latest expedition of the Carnegie Institution was printed in *Discovery* for September. Among the finds were two finely sculptured altars which date to an early period in Maya history, and a number of unevenly rounded blocks of stone carved to represent animals. One of the latter weighs about 20 tons, and shows a crouching monster with a seated human figure in deep relief in front and a great grotesque mask behind.

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An outline of the proceedings of the Congress of the International Association for Iranian Art and Archaeology held at Leningrad was printed in *The Times*, 11 September. In connexion with the Congress an exhibition was arranged to illustrate the influence of Persian art in spheres outside Iran, and an extraordinarily interesting collection was brought together from museums all over Russia. The next Congress will be held in Paris in 1937.



Dun Ruadh, the ancient burial place of Irish kings, in Crock Valley between Cookstown and Gortin, which contained at least thirteen stone cists in its chambers, has been under examination, and the original passages traced and cleared. (*Northern Whig*, 27 September).



In a note on cave-exploration by M. de Joly in *La Nature* (1 August) particulars are given of the Spéléo-Club de France, which has been actively engaged in spæleological work since its formation in 1930. The members have visited no less than 600 caves, mostly in the Basses-Alpes. M. de Joly also describes the great cave at the foot of the Guadalupe Hills in New Mexico, where a number of enormous chambers were found. In Italy too there has been great activity, about 3000 caves having been explored. (*Nature*, 14 September).



The Presidential Address to Section H of the British Association by Sir Arthur Smith Woodward on 'Recent progress in the study of Man' forms a valuable resumé of the discoveries associated with Prehistoric Man in the several continents. A summary of the address was given in *Nature*, 14 September.



An expedition of the Moscow State Museum of History has been excavating in the Altai Mountains of Mongolia, where a number of burial-grounds have been found to contain the remains of men and women, together with those of one, two, or more horses. They are said to date from 7th-9th centuries. (*Morning Post*, 16 September).



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A very fine example of Hittite art in the form of a silver bull mounted on bronze, and inlaid with gold, has been exhibited in London. One authority dates it as of the third millennium B.C. A description of the figure is given in *The Times*, 16 September.



The remains of a Roman fort have been uncovered at Brampton, where Mr F. G. Simpson and Mr I. A. Richmond have been excavating. The site forms part of Brampton old churchyard. The walls of the western granary show some particularly fine masonry. (*Carlisle Journal*, 13 September).



Chichester City Council proposes to purchase the site of the Roman amphitheatre recently found there and plans for systematic excavation have been made. We congratulate the City Council for its interest in local antiquities, and for encouraging their investigation. (*Sussex Daily News*, 4 October).



Notes on recent archaeological research in Western China, where a good deal of solid work has been accomplished in the last few years, are printed in *Nature*, 31 August.



Interesting results are reported from the excavations at Sheepen Farm site, Colchester, where a 1st century Romano-Celtic temple was discovered. (*The Times*, 10, 28 August, 3 September).



Useful work has been done near Athens by Professor Photiades and other Greek archaeologists at Koumoundouros Hill, halfway between modern Phaleron and the Piraeus. The hill overlooks a small harbour used for yachting and fishing. In the course of digging the foundations for a club-house evidence of ancient remains justify the theory of Professor Photiades that the headland was the site of the

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acropolis of King Munychos, and that the yacht-harbour was the ancient port of Phaleron. Details of the excavations, and of some inscriptions, are given in *The Times*, 6 August.

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A well-illustrated account of the inscriptions from Tell Duweir (Lachish), found by the Wellcome Archaeological Expedition in the Near East, is written by the Director, Mr J. L. Starkey, in the *Illus. London News* of 10 August. The characters are on a ewer from a temple of the 18th-19th dynasty (1400-1260 B.C.) and the script appears to be connected with those of Ras Shamra. A table is given showing the different forms of the characters used.

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Among articles in the *Illus. London News* on recent work in the East are those by Dr Henry Frankfort on discoveries at Tell Asmar and other sites in Iraq (14 September), and the first account (21 September) of the Ishtar-Kititum sanctuary at Ishchali, dating from about 1900 B.C. Among the plaques found are two of the goddess Ishtar-Kititum, one representing her with large necklaces and the other standing in a flounced robe, between two minor gods. A remarkable fragment of a carved stone vase with a figure of a moufflon, and the original inlaid eye, of shell, replaced in the socket, is illustrated.

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Illustrations, including an air-photograph, of the Khorsabad site in Iraq, which is being excavated for the Iraq Expedition of the Oriental Institute of Chicago, are given in the *Illus. London News*, 28 September, with a descriptive article by Mr Gordon Loud, the Director. They show the interior decorations, a storage chamber for water, a conduit of the water-supply system, and ivory fragments with designs for decoration.

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The excavations which have been in progress since 1933 at the Roman Villa at Rudston, 6 miles west of Bridlington are reported in *The Times*, 22 October. The Villa is assigned to the 3rd century.



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An interesting feature is a complex ditch-system which lies under the Roman buildings. One ditch runs north and south below the mosaic pavements and two others approximately east and west. The existence of a pre-Roman construction is confirmed by pottery of a late Iron Age type.

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Recent excavations at the Roman fort at Brancaster, conducted by Mr K. St. Joseph, assisted by Mr R. R. Clarke, show that the defences of the fort—which is almost exactly a square of  $7\frac{1}{2}$  acres (within the walls)—consisted of a wall  $8\frac{1}{2}$  ft. thick with a 20-foot rampart behind, and in front after an intervening berm of some 30 ft., a ditch about 30 ft. wide and 7 or 8 feet deep. The wall was composed of flint, hard chalk rubble and concrete, faced with sandstone blocks; the rampart of sand and earth, probably mainly the material from the ditch. There are seemingly no bastions; the corners are turned in simple curves, with internal rectangular corner-turrets. There are central gates in the east and west sides, most probably also in the north and south. The two former apparently had guard-chambers projecting in front of the line of the fort walls.

Though the area and proportions of the fort agree with the Saxon Shore fort type, the style of the defences conforms much more closely to the normal 2nd-century practice. Within the fort two periods were recognized, the later being represented by rough floors of rammed chalk and very rude sandstone walls. The small finds from the make-up of the later floors include material attributable to the early and mid 4th century.

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In our last number we published a note on threshing-sledges in Cyprus. We were quite unaware that a very similar note by Mr James Hornell was printed (no. 112) in *MAN*, for August 1930, and we take this opportunity of thanking him for calling our attention to it, and to express regret for the oversight.

## Reviews

THE CELTIC CHURCH IN SCOTLAND: a study of its Penetration Lines and Art Relationships. By W. DOUGLAS SIMPSON. (Aberdeen University Studies no. 111). Aberdeen University Press, 1935. pp. 120, 25 figures, 6 maps. 6s.

This is a history of the conversion of Pictish Scotland, written from a particular point of view which the author summarizes as follows (pp. 116-7): 'So far as the Picts of the north-east are concerned . . . this great work was achieved mainly by Britonic and Pictish emissaries from *Candida Casa* of St. Ninian, Bangor of the Irish Picts, Kingarth and Glasgow of the Britons, and their local daughter houses, and not to any major extent by St. Columba and the Scotie Church of Iona'. Dr Simpson allows (p. 68) that the documentary evidence for the history of these early missionaries is exceedingly meagre and often centuries later than their time, but in view of this he has devised an ingenious alternative method of enquiry. It is well known that early Celtic churches and monasteries were as a rule called by the name of their real founder or of his own monastic patron, and the author believes that by plotting distribution-maps of such ecclesiastical sites the movements of the missionary who founded them can be traced and his starting point determined. By this means he shows that the conversion of the whole of Pictish Scotland was the work of Ninian and his successors at Whithorn, and of Irishmen from monasteries founded in Ireland by his disciples, and that St. Columba's fame as 'apostle to the Picts' has been grossly and often deliberately exaggerated. He treats the whole movement as one ultimately derived from and inspired by Roman Britain and the Roman church, converting pagans and strengthening Christians in a Scotland already very largely Romanized by contact with the British province; and supports it (chap. 8) by his theory that the equal-armed crosses of east Pictland (as well as the Anglian crosses of the round non-wheel head type<sup>1</sup>) were evolved at Whithorn out of the early Chi-Rho cross and that the design was carried with them northwards by the Ninianic missionaries.

All this forms an interesting and stimulating hypothesis which Dr Simpson propounds with much learning and a studied impartiality. Unfortunately historians, archaeologists, and students of philology and literature will find a good deal to object to in his interpretation of the evidence.

Dr Simpson is right to stress the fact of Roman trade contacts with barbarian Scotland, but he goes too far in this. The tendency of recent archaeological research has been on the contrary to emphasize that much which was native and Celtic survived the Roman occupation of Britain and that the 'Romanization' school had overstated its case; the author's archaeology dates back to Haverfield. Treating Ninian as an emissary of Rome to the (supposed) Romanized communities of Scotland and his mission as a deliberate piece of Roman policy, he would show that the foundations of Ninian coincide with the known Roman sites and sphere of influence in Scotland, and that too because he was a Roman working among quasi-Roman people. Actually the first thing that strikes one about Dr Simpson's distribution-map (fig. 1) is that out of the eight Ninianic foundations marked only one is closely associated with a Roman context and

<sup>1</sup> e.g. those figured in Collingwood's *Northumbrian Crosses*, p. 64.



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three are very conspicuously far away from anything of the kind. The rest are ranged along much the same strip of country as that followed by Agricola's road, and doubtless for the very good reason that geographical considerations make it the one natural line of penetration towards the north. Dr Simpson half realizes this: 'Christian missions into Pictland tended to follow a well defined strategic route, dictated by the fundamental and inevitable geographical conditions' (p. 77). Distribution-maps can be tricky things. For example, 'that St. Donnan commenced his journey from *Candida Casa* is nowhere stated by any ancient author, but it is rendered exceedingly probable by the range of his churches . . . which . . . exactly follow the footsteps of St. Ninian' (p. 66). Reference to the map (fig. 9) shows that foundations named after Donnan are concentrated down the western coasts and the islands, with the exception of one on the line of the Caledonian Canal, one in east Sutherland, and one in northeast Aberdeenshire. This suggests rather that Donnan's journeyings were all by sea, with perhaps one excursion up to Inverness and the northeast, and 'exactly follow the footsteps of St. Ninian' is hardly an accurate description. But what evidence does this or any other 'track chart of the saints' (p. 66) give us of where the saint started from and where he finished? As for the attack upon Columba, the author repeats the arguments from his *The Historical St. Columba*, and one need only refer to Professor Watson's criticisms of that book,<sup>2</sup> criticisms which are not satisfactorily answered here.

There are certain other assumptions which cannot pass unchallenged. Ninian may have been a Roman Briton and Whithorn an offshoot of the Roman church, but it is a little strange to claim that the later Irish missionaries to the Picts from monasteries founded by Ninian's disciples in Ireland generations before, were any more Romanizing than Columba himself. One notes too implied the utterly unfounded suppositions that Irishmen of so-called *Cruithne* descent were in some way more acceptable to the Picts of Scotland than any other Irishmen, and that they spoke and understood the Pictish language. For the theory that the late cross-types derive from the Whithorn Chi-Rho monogram, the unbridged gap of hundreds of years between is a serious objection; but in general there are some interesting observations in this chapter on crosses.

The least satisfactory part of the book is the account of the post-Roman period in Britain, because Dr Simpson has been content to accept the theories of obsolete and often very untrustworthy authorities such as Skene, Nicholson, and Rhys, without examining the evidence more closely. He clearly has no acquaintance at first hand with the Celtic languages and their literatures. It is not good enough to excuse oneself on the plea made by Haverfield in 1907 that Celtic philology is in a state of chaos and is 'no place for a respectable historian' (p. 7). If that was true in Haverfield's time it has long since ceased to be so, and on the contrary it is the business of a 'respectable' Celtic historian to master the languages so that he can use their testimony with understanding. Thus his treatment of Cunedda is quite unsound, based partly on Rhys's misunderstandings of a poem in the Book of Taliessin—there is of course nothing to show that Cunedda 'had his headquarters at Carlisle', that 'he is represented as wearing . . . the special badge of a Roman *Dux*', or that his son Ceredig was the same person as Ceredig Wledig of Strathclyde (1); these things are unfounded speculations. The same applies to much of the section on Rhydderch Hael and the battle of Arfderydd, pp. 85-6. Again, the author ignores the Gododdin poems and makes some strange statements about the Otadini in consequence (p. 62). The passages on the Saxon invasions and settlement (pp. 74-5, 102-5) leave much to be desired.

<sup>2</sup> *Aberdeen University Review*, vol. 15.



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Finally, some individual points. The following bastard forms: Eddi for Eddius or Aedde, Maglocune for Maglocunus or Maelgwn, Hy Niall for Uí Néill, Gwenddoleu for Gwenddoleu, Keledci for Céili Dé (p. 116; *Céile* does not mean 'servant' in this phrase, but 'vassal'). *Tóthail* (p. 61) is Irish; it is the genitive singular (after *filius*) of *Tóthal*, the archaic form, contemporary with Adamnan, of the later *Tuathal*. P. 67, note 2, it is correct that the Martyrology of Oengus was composed about 800, but not that the notes to it were; hence the story of Donnan's death is even less likely to be true. P. 102, there may very probably have been an 'Artorius, *Dux Bellorum*', but it is only a hypothesis. P. 62, note 1, a case of wrong reference—for Cymmrodorion Soc. read Y Cymmrodor, vol. XXI, 63-104.

Dr Simpson's views on the conversion of the Picts would be more authoritative if we could feel sure that they are not influenced—absolutely unconsciously of course—by that pro-Pictish feeling, anti-Gaelic and anti-Saxon, which is apt to distort the outlook of modern 'Pictish' writers. Yet cautious speculation has its value, and the book is well worth reading for that reason.

KENNETH JACKSON.

THE STORY OF BITUMEN. Published by SHELL. pp. 63 and 37 illus. No date, place of publication, or price.

AUS DER ÄLTESTEN GESCHICHTE DES BITUMENS. By R. J. FORBES, Amsterdam. Extract from BITUMEN. Berlin, 1934. pp. 15 and 19 illus.

These two papers are apparently by the same author and cover the same ground, using the same illustrations. They record the history of bitumen from the earliest times, with all the existing ancient references from Greek and Roman authors and all possible archaeological illustrations. In effect the compilation is one which will be of great use to archaeologists, for it contains all relevant material set out in precise and convenient form. The medieval section is also full and instructive. The use of bitumen for damp courses in reservoirs and cisterns in India at Mohenjo-daro and in Mesopotamia at various periods, its use for bonding bricks in Babylon, and at Ur as a backing for mosaic are among its more important archaeological purposes. The author gives a most useful and well-documented account of 'Greek Fire' which was a preparation based on a secret formula which produced a self-igniting mixture of petroleum and quicklime. The invention passed from Byzantines to Arabs and thence in the 8th century to the Chinese.

The Mongols employed small balloons filled with hot air produced by petroleum lamps. These balloons were the true predecessors of Montgolfier's balloon, though by the Mongols they were used solely for signal purposes.

S. CASSON.

THE STONE AGE RACES OF KENYA. By L. S. B. LEAKEY. Oxford University Press, 1935. pp. vii, 145, 37 plates, 52 figures, map and 6 appendices. £1 18s.

This work is an outcome of the labours of the East African Archaeological Expedition (seasons 1926, 1928 and 1931) and except for its greater size and larger bulk, the volume is in every way a companion to *The Stone Age Cultures of Kenya Colony* by the same author. It is a remarkable book, and the name of the publishers is sufficient guarantee of excellent production. It is almost lavishly illustrated, thus serving admirably to support and elucidate its title.

As it must find a place on the shelves of libraries to which specialists in human osteology, prehistoric anthropology and kindred subjects repair for purposes of study



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and reference, its high price, which puts it beyond the purchasing ability of most students, is less of a drawback than it might at first appear. But it is a very great pity that the most remarkable and interesting of the relics found by Dr Leakey and his co-workers, described in such detail in this volume—the Kanjera and Kanam human remains—must be dethroned from their unique positions, the one as the earliest of all *Homo sapiens* and the other as an amazingly ancient *sapiens*-like *Homo*, because judgment with regard to their geological age must be held in suspense. (*Nature*, 9 March 1935, p. 371). It is safe to say, however, that neither is modern, and judging by the nature and degree of its mineralization the Kanam mandible is decidedly ancient. A discussion of anatomical details would be out of place in ANTIQUITY, but those who wish to know the considered views of a leader in the study of prehistoric anthropology in the present connexion will do well to turn to the pages of *Nature* (2 Feb. 1935, p. 163).

The author does not, however, confine himself in this volume to anatomy. Dr Leakey sets forth his view with regard to Pleistocene climates and topographical changes in Eastern Africa, and it is essentially to him that we are indebted for our knowledge of post-pluvial (post-Würmian?) climatic phases in Kenya.

It has been shown that there were two main pluvial periods in Eastern Africa during the Pleistocene followed by a relatively dry spell, and then came a moist period (the Makalian), another dry spell and then a moist one again (the Nakuran), and Dr Leakey is able to tell us much concerning the peoples who experienced the humanly important climatic changes from Upper Pleistocene to Neolithic times in Kenya.

Dr Leakey calls the first pluvial the Kamasian and the second the Gamblian. According to one interpretation, however, his Kamasian includes the first part of the second pluvial as well as all of the first. But in addition to meteorological vicissitudes, the Pleistocene of Eastern and Central Africa has witnessed very important changes of topography consequent upon movements of the earth's crust and, in Kenya, vast volcanic outpourings.

It is not clear, however, what Dr Leakey means by 'volcanic upheavals', nor by 'earth movements of a less magnitude' which, it is stated 'have occurred from time to time, chiefly as a result of the settling down of the disturbed deposits' (the italics are mine). Nor will everyone agree that 'ice sheets extended to all levels over two thousand feet' and that 'in order to explain the ice-sheets we have to postulate that there are periods of increased precipitation combined with a lowering of temperature' (again the italics are mine).

Dr Leakey has something to say about the effects of present-day climatic conditions and states that 'erosion in the Kanam-Kanjera area is going on at present at an incredible pace', but comparison of photographs taken a few years ago with the ground they depicted does not support this view.

Dr Leakey is hardly correct when he states 'I have also shown that the lowering of the lake [Victoria] was due to the formation of the outlet at Jinja, as a result of earth-movements. Once the Jinja outlet had been formed the lake could never rise appreciably again, no matter how great the increase of rainfall'. None-the-less it did!

According to the evidence, as we now know it, the Jinja outlet was formed not at 'the end of the Kamasian Pluvial period' but long before that, and the level of Lake Victoria was controlled not by that outlet but by the original discharge channel which was the Kufu valley. Late in Pluvial II times, however, this drainage was diverted along the present lower Victoria Nile and reversal and accelerated erosion along the new course and rejuvenation along the Upper Victoria Nile, downstream of Jinja, started.



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It is a pity that the map at the end of the volume is so out of date and that the remarkable drainage systems of Uganda so obviously (when rightly depicted) and so intimately bound up with the history of Lake Victoria are so inadequately and/or wrongly shown, for they provide the first clue to the remarkable physiographic history of Eastern Central Africa during the Pleistocene. These criticisms, however, hardly detract from the value of the book as a fitting record of some very remarkable researches and discoveries which have opened up a new chapter in the story of Stone-Age peoples. It is a record, moreover, which no prehistorian can afford to neglect. E. J. WAYLAND.

THE CONSTITUTIONAL HISTORY OF THE CINQUE PORTS. By K. M. E. MURRAY. (Publications of the University of Manchester, CCXXXV, Historical Series, LXVIII). pp. xvi, 282 and map. Manchester University Press, 1935. 10s 6d.

The Cinque Ports have long deserved a history that shall do more for them than Montague Burrows could have done, and at last they have got it. True, it is a Constitutional History; but Miss Murray has refused to be too orthodox in her definitions and the result is a fairly general and generous survey of this cluster of federated towns. Yet it is more than a mere survey. This solid and handsome volume, which is likely to be the standard work on the Cinque Ports for many years to come, has been built up on a thorough-going scrutiny of the original documents and a shrewd appraisal of their content and implications. The author's arrangement of her material is well planned and the style is distinguished, fluent and worthy of her subject.

For those who take the romantic view of the Cinque Ports, or who have been brought up on Burrows, there will be some disappointment. Miss Murray's book has undoubtedly the effect of diminishing our sense of the historical importance of the Five Ports, the two Ancient Towns, and their Corporate and non-Corporate Members, either as individuals or a confederation. Nevertheless, she is correct in deciding that their part in naval affairs has been over-estimated, their liberties exaggerated, and the origins of their joint organization considerably ante-dated.

The status of the Cinque Ports as a federation of towns—the only example in Britain—was not that of a self-contained and independent unit. Its Courts of Shepway and Brodhull, though possessed of special privileges and powers, were not immune from outside interference. They certainly helped to bind the towns and barons more closely together, and the Brodhull was, from 1357 at any rate, the general assembly of the Cinque Ports. On the other hand, the very existence of the Court of Guestling is a proof of the looseness of the confederation and of the tendency of the Ports to separate into county groups. The office of Warden of the Cinque Ports, though a position of great profit and prestige, was not the kind of small sovereignty that tradition asserts it to have been. Holding his commission at the king's pleasure, the warden was also bound by oath to uphold the liberties of the Cinque Ports; thus being nicely maintained in an equipoise between the two. In the 13th century the Cinque Ports were vital to England; in the 14th, their services were of value; after that came the decline.

If the above appears to be a summary of negations, the book is a great deal more than that. While traversing the questionable arguments and assumptions of the older historians, it covers the whole ground of the Cinque Ports constitution. In order to do so, it has had to forego the consideration of some aspects of their history that are always of a lively interest. The details of naval achievements; the identification of the sites of Shepway and Brodhull; discussions on the absence of a common seal, the origin of



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the common flag, and the heraldry of the common arms—these, however welcome, would have been gratuitous in a Constitutional History.

It is a pleasure to handle a volume so well printed and bound, and with so good an index. The fact that a few slight discrepancies of punctuation (see pp. 83, 127, 134 and 136) have caught the eye is really a tribute to the excellence of the proof-reading.

FRAZER HEARNE.

**SLEDGES AND WHEELED VEHICLES:** ethnological studies from the view-point of Sweden. (Nordiska Museets Handlingar: 4). By GÖSTA BERG. Uppsala: *Almqvist and Wiksells Boktryckeri-A-B*, 1935. pp. 189, 32 plates, 38 drawings, 13 distribution maps. Price not stated.

'The object of this work', says its author, 'is to investigate problems connected with the history of vehicular transport from a Swedish point of view'. But, though he is thus an avowed specialist, he never loses sight of the two facts, that Swedish transport is part of the world's transport, and that vehicles are historically important because they are an essential part of the culture of their users. He is to be congratulated on treating studies of vehicles as the ethnological studies that they certainly are.

Besides dealing very fully with what may be called the 'normal' stages of slide-car, sledge, wheeled-sledge, cart and waggon, he produces evidence of a pre-sledge era of single runners dating back to neolithic times; and he also devotes much attention to the difference between the pair-drawn and the single-draught forms of cart. Much that is said bears on points that have been already raised in *ANTIQUITY*.

The vast wealth of evidence accumulated in this book forms in itself a permanent and valuable contribution to the literature of the subject. The plates provide nearly a hundred good photographs and reproductions. The drawings are excellent, but the value of the distribution-maps would be increased if we were told more about the methods by which they were drawn up, and if the symbols in some of them were more clearly explained. A minor defect is that several illustrations are wrongly numbered in the text.

Such theories as are expressed are, wisely, somewhat tentative. The author recognizes, as all must who tackle this subject seriously, that both wider and more thorough researches are needed before a complete statement can be made with any approach to certainty. He does, however, commit himself to the unqualified opinion that the single-horse cart had its origin in the slide-car. More often he plays the part of the research-worker, with a patience and thoroughness which compel admiration.

It must be recognized that the translator had to deal with an extremely difficult task. But this does not alter the fact that the English reader is faced by obvious mis-translations in several rather important passages; these not only baffle him in themselves, but also raise uncomfortable doubts about the accuracy of other passages. The sum total of these errors and possible errors is, however, only a small fraction of the whole.

The paper and print are good, and the book, when opened, lies flat. This is helpful to the reader, particularly in a volume which one is likely to wish to leave open for comparison with others. A bibliography of the subject occupies 17 pages.

There is no doubt that this book will be of great value to anyone seriously interested in the history of transport. It will provide English readers with information that they are not likely to be able to get for themselves. It is to be hoped that someone will produce a similar book, dealing with these matters from 'the view-point of Great Britain'.

R. H. LANE.

## ANTIQUITY

CORPUS VASORUM ANTIQUORUM. United States of America. Providence : Museum of the Rhode Island School of Design. Fascicule I. By STEPHEN BLEECKER LUCE. pp. 50 and 31 plates. 20s.

UNIVERSITY OF MICHIGAN. Fascicule I. By WILHELMINA VAN INGEN. pp. 84 and 48 plates. 20s.

THE ROBINSON COLLECTION, BALTIMORE. Fascicule I. By DAVID MOORE ROBINSON. pp. 58 and 48 plates, 9 text-figures. 35s.  
*Harvard Univ. Press and Oxford Univ. Press, 1933-4.*

These three volumes of the ceramic Corpus are a notable contribution to this great international undertaking. The Rhode Island collection comprises a good working selection of Mediterranean pottery of all types. Egyptian prehistoric and historic wares are well represented and the single example of Mesopotamian ware from the Tigris-Euphrates basin is of a type not often seen in European or American museums. Cypriote ceramic is very poorly represented and there is little Mycenaean. The main strength of the collection lies in Attic wares of the finest periods. Twenty-five vases from the Warren collection purchased in 1925 increase the value of the Attic section. Indeed Providence can show both the eponymous vase of the 'Providence painter' (as might be expected) and a good half-dozen first rate examples of the work of the leading Attic painters. Corinthian pottery is poorly represented and proto-Corinthian a lacuna, but a lovely Rhodian orientalizing oenochoe and a Chalcidian oenochoe fill what would otherwise be serious gaps in the whole series. The Dipylon group of five vessels is well above the average in quality.

Of the Attic wares the early amphora with two confronted male figures in a spacious field shows the admirable restraint of the first attempts of Attic painters of genius. The Nikosthenes amphora (pl. 9), signed by the artist, is one of the finest vases in America; its shoulder-frieze of horsemen and standing figures shows the same reserve and balance as in the preceding vase but has in addition the added glory of a superb architectural design on the main body of the vase. The column-crater on pl. 11 shows the work of two quite different painters on the two sides: the small lekythos with the lovely rendering of Europa and the Bull is far above the level of small work in black-figure.

Among the red-figure vases we have a new attribution by Mr Luce of a fine Nolan amphora to the Nikon Painter (pl. 13, 1a) and another to the circle of the Dionokles Painter. Two lekythi showing Danae in the chest and a potter firing a vase in a kiln are of unusual subject-interest (pl. 17). The amphora which gives its name to the Providence Painter shows one of the stateliest representations of Apollo with his cithara to be found in Greek vase-painting. A small lekythos (pl. 19, 2) seems to be also a work of this painter, and the Brygos lekythos showing a seated Hera is of the same high level of execution as the preceding works. The white lekythi are poor in quality and the example illustrated in the coloured plate hardly deserves that honour.

The Michigan collection, in contrast, is wholly undistinguished, but is a useful collection for students. It contains no work of any kind of artistic merit, but well illustrates the daily wares of the ancients, from protodynastic Egyptian times down to the days of Roman Gaul. A group of Ptolemaic faience will be useful for students of these ornamental wares and their later derivatives, and a small group of Yortan vases gives a ware otherwise hardly seen in American museums. A very odd fragment (pl. 4, 5) from Pisidian Antioch shows a unique design in Mycenaean ware. It may well come into its own when more micrasiatic sites are excavated: at present it stands alone. A



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fine Cypriote series covers the Bronze and Iron Ages but stops short of the orientalizing period. The Attic wares are poor indeed, but some black figure-vases have merit.

The Robinson collection is well above the average of the Michigan in quality and is an admirable record of the careful search on the part of one collector, whose intention was to make a good representative series for students that maintained a high artistic standard. The high spots of the collection are to be found in the lovely Attic white-ground pyxis, perhaps by the Penthesilea Painter (who is now well represented in America), in an unusually good white lekythos by the otherwise dull Painter of the Bowdoin Box, and in a black-figure hydria by the Acheloos Painter. A group of Thessalian sherds and a group of Panathenaic vases of the third quarter of the 5th century are of historical value for students; the latter in particular, since they help to enlarge our knowledge of this archaizing series of works of art. The combination of an archaistic Athena and athletes who are non-archaic is a strange amalgam. Their date cannot be so easily fixed as Prof. Robinson would suppose. A rectangular altar from Skione is an interesting extra; whether it is an actual portable altar or a mere base for a small statue is uncertain. It should be compared with a recently found 'altar' of the same type from Corinth. The figure of a silene painted on one side is good work.

The series of white lekythi are mostly of the late and sentimental period and cannot rank as more than pleasant ornaments. Beautiful white lekythi are few and far between.

S. CASSON.

### CATALOGUE OF ANTIQUITIES IN THE MUSEUM OF THE WILTSHIRE ARCHAEOLOGICAL AND NATURAL HISTORY SOCIETY AT DEVIZES.

Part II, 2nd edition. *Compiled by MRS M. E. CUNNINGTON and the REV. CANON E. H. GODDARD. Published by the Society at the Museum, Devizes, 1934. (George Simpson, Devizes). pp. xiv, 294, 92 plates and 46 illus. 2s 6d (postage 6d).*

Those who have a knowledge of the previous works of the compilers of this Catalogue, including the first edition thereof, will not be surprised to learn that the second edition is a first-class production. Though modestly entitled a Catalogue, it is in reality far more than that; it is a mine of information for anyone interested in the science of archaeology, apart from its value to a visitor to the Devizes Museum, and it should be in the library of every archaeological student.

It deals with all the antiquities in the Museum, with the exception of those included in the Stourhead Collection, of which there is a separate catalogue, part I. The first 256 pages are concerned with the specimens dating from 'Eolithic' times to the Christian-Saxon period; the remaining pages deal with objects of Medieval and later date, and with certain miscellaneous exhibits and models. There is a short addendum.

It is well known that Wiltshire excels any other county in the British Isles as regards the number of its barrows, long and round; it is therefore not surprising that the Devizes Museum is exceptionally well furnished with grave-goods of the Neolithic and Bronze Ages, which are thoroughly well described in the catalogue. The objects from the many Iron Age habitation-sites and earthworks have their full share of notice, and there is a very adequate description of the Roman and British antiquities.

A feature of the Catalogue is the excellent, concise accounts of the excavations which furnished collections of objects from particular sites. The general arrangement leaves nothing to be desired, and the plates and illustrations are very clear and useful. Last, but by no means least, there is an excellent index, so that there is no loss of time in obtaining all the information that is available on any object in the Catalogue.

A. SHAW MELLOR.



## ANTIQUITY

THE VICTORIA HISTORY OF THE COUNTY OF RUTLAND: Vol. II. *Edited by the late WILLIAM PAGE. London: St. Catherine Press, Stamford Street, 1935. pp. XLIV, 284 with plates and text-illustrations. 63s.*

This volume, the ninety-third of the series, completes (except for the index-supplement) the survey of England's smallest county, and it may be said at once that it fully maintains the high standard already set for this work. Moreover, it possesses a special interest for many readers in that it represents the last volume issued under the general editorship of the late Dr William Page, F.S.A., an admirable memoir of whom, from the pen of Sir Charles Peers, is included therein, together with a portrait. The present volume is concerned with the topography of the county and contains a large amount of valuable and well-ordered information on each hundred and parish within it, representing the results of much patient research on the part of Dr Page's staff of helpers. The descriptions of the parish churches have been undertaken by Mr F. H. Cheetham, F.S.A., who has carried out his task with commendable care and thoroughness, while the later domestic buildings and the heraldry of the county have been dealt with by two acknowledged masters of their subjects, Mr J. A. Gotch, F.S.A., and the Rev. E. E. Dorling, F.S.A. A word of special appreciation is due to Miss Jamison, who is responsible for the introduction. Here, in some 16 pages, she has succeeded in giving a really excellent summary of the more important material contained in the succeeding sections and thus provides the general reader with some idea of what Michael Drayton calls 'the rarities that Rutland shows'. (*Polyolbion*). The illustrations in the volume are uniformly good.

V. B. CROWTHER-BEYNON.

ATTIC VASE-PAINTING. *Martin Classical Lectures, vol. III. By C. T. SELTMAN. Harvard University Press, 1933. pp. 89 and 37 plates. \$1.50.*

The cheap price of the series issued by this Foundation is much to be commended. I know of no archaeological work of such merit which has been issued in any country at so low a price. The Martin Foundation is carrying out its trust faithfully.

Mr Seltman writes a most lucid and instructive book. After a preliminary discussion on art in general and Greek art in particular, in which he clears the air of many obscurities, he warns us briefly that the popular idea of Greek art is moulded on 'fleshy female types like the Venus de Milo or monsters like the Laocoon' and then proceeds with his exposition. His account of Geometric art, which he calls 'of a textile type', is clear and correct. Geometric art may have been Dorian in inspiration but the Attic artist gave it a quality which made it into something 'overwhelmingly impressive'. The next phase, about 600 B.C., is that in which we see Attica 'despite political insignificance, asserting in art an unconscious claim to the spiritual leadership of Greece'. Soon Attic wares became world-famous as works of art, not as receptacles of merchandise—'Chinese vases did not come to 17th century Europe because the Europeans had a passion for ginger and lygees, but because Ming was better than Majolica'. And so Mr Seltman enters the field of individual painters and potters whose styles are fully known. He describes their characteristics and their artistic connexions and makes a simple story out of a maze of facts which the student often finds it hard to assimilate. Nor does he forget to explain the development of painting on vases in relation to the development of the major arts. He pursues his subject to its end and describes the vulgarism of Meidias and Aristophanes, the last of the vase painters, before the art perished for good, before the fifth century had ended.



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Mr Seltman is to be congratulated on a book which in small compass covers the whole ground with precision and simplicity. His illustrations are above reproach. His date for the introduction of orientaling influences is perhaps too late: his suggestion that the *kantharoi* depicted on vase paintings are probably of metal can now be well illustrated by the two superb silver *kantharoi* found in Bulgaria. There is little else to criticize and to add to a work thus competently achieved.

S. CASSON.

ESSAYS IN CORNISH HISTORY. By CHARLES HENDERSON. Edited by A. L. ROWSE and M. I. HENDERSON. Oxford: Clarendon Press, 1935. pp. XXIV, 241. 12s 6d.

By the untimely death of Charles Henderson, Cornwall—and England—has lost one who could have done great things. For Henderson stood in the direct line of succession to the great old county historians; he was of the company of Colt Hoare, Hutchins and Hasted. It is a great tragedy that, instead of the great History of Cornwall which he designed to write, there should remain for posterity merely these essays, described by one of the editors as 'the bare stuff of his knowledge, not its final construction'. His 16,000 ancient documents now lie in the Museum at Truro with many hundred transcripts of his hand, the bequest of a notable historian, but still a mere dump of material'. . . . 'There the artillery will lie. But who now can bend his bow?' says Sir Arthur Quiller-Couch, in a charming and adequate preface.

But fragmentary though they are, these essays are of golden quality. They reveal the sure touch of the master-craftsman. Whether he was describing the borough records of Truro or Cornish place-names or the gardens, deer-parks, ferries, inns, wrecks and light-houses, bells, crosses or old estate-maps of Cornwall, Henderson gave life to the dry bones of the past. And why? For two main reasons; primarily because he always wrote from first-hand knowledge of deeds or land, but also because he was not merely an indoor student (though he was that in full measure). He knew and loved the land of Cornwall, its windy moors and coasts, and its wooded combs; he was a born topographer and field-worker, and related his knowledge of written documents to the land itself. Moreover, the past in which he lived was a real thing; it was the Cornwall he knew, transformed by the magic touch of an informed, historical imagination. Such talent is rare, and the legacy here bequeathed to the public will be read with genuine, if regretful, pleasure by all who care for true scholarship.

O.G.S.C.

FOUILLES DE TELLOH: sous la direction de H. de Genouillac. Tome 1. Epoques Présargoniques. Paris: Geuthner, 1934. Tomes 1-11, 300 frs.

'C'est Telloh qui nous a révélé les Sumériens'. These are M. de Genouillac's first words in his preface, and they are a fitting tribute to the magnificent work done by the French for the last half-century on this Mesopotamian site, begun by De Sarzec, and carried on by numerous other distinguished savants. Since 1929 the expedition has been under the control of M. de Genouillac, and he has now reached a level fourteen metres below that of De Sarzec's work on the buildings of Entemena. His finds are, as would be expected, similar to those made in other prehistoric strata in the neighbourhood; but although the remains at water level are of the same kind as those found by Woolley at Ur, no 'Flood' has been revealed. This is a curious hiatus (it was equally so at Warka), since Watelin has maintained the existence of a 'Flood' at Kish, in addition to Woolley's at Ur.



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It is, of course, to the painted pottery to which our attention is naturally turned at the outset, and I see that M. de Genouillac supports the theory which I held when finding the black-painted ware at Abu Shahrain in 1918, that it is not Sumerian. I confess to being still of the same opinion: I cannot see how it can be possible for those near neighbours of the Sumerians, the Ninevites, to show such a quantity of the black-painted ware which can be brought into relation with much of that found on Sumerian sites (that is to say, Nin. 4 and 5), and yet practically nothing to show a connexion with the Sumerians in art or language. At Nineveh there are no native Sumerian brick inscriptions to show Sumerian occupation, and the very rare Sumerian objects may well have been due to trading.

With these 'finds' from Telloh the prehistoric archaeologist has now another collection of pottery for his consideration, and he will find it admirably catalogued and photographed herein. They do not, perhaps, reveal anything of very new and popular interest, but among them there is one outstanding fragment of potsherd with an unusual incised drawing of a man of the period. It is, perhaps, a little difficult to sympathize with the words *coup de poing* in association with a specimen of the numerous stone hoes which are always found on these sites, and the learned author trips once in saying 'l'homme de la céramique peinte . . . ne figure point de dieux ni de démons comme des hommes'. He will find a wonderful picture of a headless vampire on a bowl from Moussian (*Délg. en Perse*, VIII, 136, fig. 266) where the sufferer, who has been visited by night by the succuba, shows what will happen to her if she return; and this queer superstition is shown on later seals (e.g., *Liv. Annals*, XIX, pl. LXIII, no. 1, and *Rev. d'Assyr.*, 1909, 61). These latter points are, however, trivial criticisms to make on a publication so admirably edited and produced, after what must have been great labours in the field.

R. CAMPBELL THOMPSON.

✓ ORIGINS AND DEVELOPMENT OF APPLIED CHEMISTRY. By J. R. PARTINGTON. Longmans, Green & Co., 1935. pp. XII and 597. 45s.

It is not often that a reviewer completes his review with the feeling that the book reviewed is, of its type, as complete, comprehensive and accurate as a book can be. This book has all three virtues and many more. It is, in effect, a massive encyclopaedia written by one person upon the vast topic of the history of material substances used by man. As such it is and will remain for a generation an essential handbook for every archaeologist. Indeed it will contribute largely to the improvement of archaeological method and research, for it does what no archaeologist can do by himself—correlates the material from various regions and discusses it from the standpoint of an expert professional chemist. The book is by no means a mere collection of data relating to material substances from excavations; it is far more than that. The author systematically covers the whole of the Old World. Egypt, Babylonia, Crete, Troy and Cyprus, Asia Minor, Greece, Persia, Phoenicia and Palestine are dealt with in turn. Each section is preceded by a condensed and admirably clear historical exposition of the area and its inhabitants. Then follows a discussion of the minerals, metals, and other inorganic substances of which we have knowledge either from excavation or from literary sources, and then of the organic substances used in industry. An account of every technical process of which knowledge survives or can be reconstructed from the extant remains is also included. The reader thus soon discovers that he has in his hands a history of the ancient world down to the end of the Bronze Age written solely from the point of view of one who is concerned with the materials used by man. The title of



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the book might well be *Materia Archaeologica*. The 529 closely packed pages of small type and the 25,000 footnote references are so well arranged typographically that the publisher has contrived to contain within one not too heavy volume what one would have expected to see emerge as half a dozen smaller volumes. Consequently the archaeologist in the field will be able to convey this book with the minimum of inconvenience and the maximum of benefit. The system of footnote abbreviations is excellent and the use of the index as a bibliography a very space-saving device. When we read in the preface that the book was compiled 'in the spare time of one person, not particularly well equipped' one feels that one is at last in the presence of a real example of the twin virtues Modesty and Industry.

It is impossible in this review to do more than indicate the principal merits and the particular passages that struck the reviewer. Throughout the book the standard of accuracy is astonishing: only one misprint was noticed. The author assures us that he has personally consulted each of the thousands of footnote references, an assurance which one would like to see made in many books.

The Egyptian section is naturally the fullest. The author raises (p. 22) the important topic of how metals were in the first place discovered; gives the fullest information as to gold mining in ancient Nubia (p. 33) and from a fresh angle attacks the archaeological myth of Transylvanian gold already dealt with by Lucas (see *ANTIQUITY*, June 1935, p. 238). Apparently the original thesis that 'antimony will combine with gold only in the presence of tellurium' is chemically incorrect. His account of the use and provision of tin is of the greatest value (p. 77). The existence of rich deposits of tin near Meshed is apparently well-documented but has not yet been fully confirmed by any recent authoritative statement. Obviously, confirmation of these deposits will affect much recent controversy and provide final evidence for a restatement of the problems that concern tin supplies in the ancient East. The problem of the manufacture of steel (p. 95) is discussed. There was apparently no inherent difficulty in transforming bar-iron into steel even for primitive people. All that was needed was a proper understanding of the use of charcoal, aided by nitrogenous organic matter, and tempering. This was no insuperable task for simple smiths. The author rapidly disposes of another archaeological myth—the alleged early use of iron or secretly hardened copper, inferred from the Egyptian facility for working exceedingly hard stones. 'Abrasive powders and almost infinite patience were' he says, 'probably the agents employed'. The facts that experiments by modern workmen on the same lines have not produced the same results is, he argues, merely proof that the processes can only be effectively carried out by men who have a tradition of centuries of such work behind them.

The reader will discover from time to time in this mine of learned information occasional gems of unusual knowledge. Thus there is obsidian in Abyssinia (p. 103) as well as in Melos and Asia Minor. One learns with surprise that the first analysis of 'Egyptian Blue' was made by Sir Humphry Davy in 1815 and that it remains 'a model for all investigations of this type' (p. 117). A full discussion of Manna gives the last word on this strange substance (p. 162).

The Babylonian section is completely up to date and most valuable. An attempt is made to identify the ancient names for various minerals and there is a complete list of the materials from Ur. The section on Crete and the Mycenaean world is equally full. The author gives a notable correction of an analysis of Minoan faience which has been hopelessly garbled and rendered 'chemically almost unintelligible' by the archaeologists (p. 328), and shows that a piece of 'iron' found at Phaestos is in fact unsmelted



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magnetite (p. 336). To the universal surprise of archaeologists the author records that he 'can find no record of the use of saffron as a dye' (p. 337). Probably the safflower was used instead. The white 'Chinese' jade of Troy is shown to be of local origin (p. 342) and the author comments on the discordant analyses of bronze at Troy.

The Hittite section is particularly admirable. The historical introduction is as clear a statement of Hittite problems as can be found and the authorities consulted seem to be the best and the most recent.

The origins of Persian gold-supply is discussed with a wealth of information (p. 402) and the author's suggestion that the legendary 'ants' who protected the gold are to be identified with the fierce mastiffs which the miners of Dardistan still employ seems acceptable.

On one solitary topic alone does the author mount his hobby horse. He restates in full the case for Phoenician trade in tin with Britain. But his arguments are purely speculative and his case, though admirably stated, does not convince. It is the volume rather than the existence of Phoenician trade that is in dispute and the absence of any evidence. Few would deny that an occasional Carthaginian ship may have slipped round to Land's End. But what may have happened is not history. One Phoenician does not make an Argosy and there are stronger claims for the handling of Cornish tin by local shipping and so overland to Massilia, so that neither Greek nor Punic can be considered as having a monopoly.

Of critical comments there is space for only a few. The *cire perdue* process is a certainty, not a possibility, in Crete (p. 335). The inlaid daggers of Mycenae are certainly not Egyptian work (p. 353). There is no mention of the great nephrite and lapis lazuli axes of Troy, and nowhere can be found a full account of the niello process and, indeed very little mention of it except on p. 531. Archaeologists are extremely vague about this matter. Evans describes the black of niello as an alloy of silver and iron. Here it is suggested, I think rightly, that it is powdered silver sulphide applied with a blowpipe. A niello worker's outfit was found near Van.

The book is a monument of erudition and patience. British scholarship is to be congratulated upon this really astonishing feat of research.

STANLEY CASSON.

**STORY OF SCOTLAND IN STONE.** By IAN C. HANNAH. *Edinburgh: Oliver and Boyd, 1934. pp. xvi, 352, and 93 illustrations.*

Mr Hannah commences this interesting study of Scottish architecture with a series of generalizations, many of which are open to comment. He says of Scotland that 'the world may be searched in vain to find another nation so interested in the understanding of literature and the arts', of the Incas and Maya peoples that 'they could not even write', and of Henry Raeburn that he is one of the world's greatest artists; of Gothic cathedrals that their real power would be gone if they were transplanted from one region to another; of Celtic art that 'it is one of the few really great achievements of mankind'. From this it can at once be gathered that Mr Hannah has not let his imagination roam far from Scotland. In consequence this book, interesting and well-informed as it is, is neither a scholarly production nor an important contribution to the study of architecture.

The chapter on the genesis of Celtic art ignores the fact that much of what is termed Scottish Celtic owes its inspiration to Northumbria, although the author is fully aware of the importance of Northumbrian art. He is equally unaware that the Scottish brochs are, architecturally, not peculiar to Scotland. He should look at Sardinia and Transjordan. His description of Samian ware as 'beautiful' needs no comment, but his



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surprise that so oriental a theme as the visit of St. Anthony to Paul the hermit should appear on the Ruthwell cross shows that he is not conversant with the various Byzantine influences at work in this island. His description of the early Scottish carved stones as 'among the most remarkable early Christian monuments that the world can show' is ludicrous exaggeration, while his suggestion that later developments of Byzantine architecture do not approach the 'glories of Agia Sophia' is simple misstatement. It is not until one is well into the body of the book that one finds that there really is something distinctively Scottish about Scottish architecture. The Norman and Romanesque periods seem hardly to have differed from the English in style. The Gothic at its outset remained stubbornly southern and, as the author says, 'there were numerous minsters on rather English lines' during the great age of cathedral building. Scotland 'did not develop as yet any very striking features of her own' (p. 88). In the 'high noon' of Gothic, Scotland was 'building a number of churches on much the same lines as England' (p. 144) and one wonders at what age the Scots began to show signs of that essentially national style about which the book set out to give a description. In the end we discover that the English Perpendicular style was the first English style not to be adopted in Scotland. But that does not mean that Scotland turned to France, for Scotland does not 'possess a single church of this period that could possibly be mistaken for a French one'. Actually in the late 15th and 16th centuries Scotland at last developed a splendid style of church and cathedral building and with the Renaissance showed a perfected style, essentially Scottish, for castles, private houses and public buildings.

S. CASSON.

DER GEIST DER VORZEIT. By R. R. SCHMIDT. *Keilverlag, Berlin, 1934.*  
5 marks, bound 6.50 marks.

The only fault that we have to find with this admirable book is a mere matter of literary taste, about which there is room for legitimate differences of opinion. The author has a way of expressing himself in short, abrupt sentences, often destitute of verbs; and this detracted slightly from the pleasure and interest with which we, personally, read his work. But our pleasure and interest were so great, that we could spare a little of them without a serious sense of loss.

A logical sanity is a leading characteristic of Dr Schmidt's work. The author has no truck with wild or even unnecessarily recondite theories. For him, Humanity begins in the Ice Age, not in some remote Tertiary epoch. For him, the *Homo primigenius* of the Mousterian and earlier stages, and the *Homo sapiens* of the later Palaeolithic, are successive evolutionary phases of the same being. Dr Schmidt's long and honourable record in practical research, chiefly in the German caves, gives him a right to speak with authority: the retracing of the steps whereby the earlier form of Man develops into the later, here set forth, is both fascinating and convincing. It is illustrated with a long and well-selected series of figures. While we may complain that some of these, though perhaps inevitably, have become a little too familiar with constant repetition—we wish that some explorer would find a few more Grimaldi negroids somewhere, so as to give a rest to the overworked 'mother-and-son' of the *Grotte des Enfants*—many others are new, or not easily accessible. These include graphic and ingenious restorations of the Laussel 'woman with a horn', and the sorcerer of the *Caverne des Trois Frères*, as those personages must have appeared in the flesh.

The prologue and epilogue, headed respectively 'Seele und Artgedächtniss' and 'die Magische Wirklichkeit', for their full appreciation may possibly call for a greater



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familiarity with the writings of Nietzsche than the present reviewer possesses : but the body of the book which lies between requires no such formidable preparation.

In the first chapter, on 'The World of the Ice Age', the author treads well-beaten paths. He tells us of the extent of the ice-sheets ; touches on the unsolved problem of their causation ; and traces their effects on the living inhabitants of the regions affected. He refuses to accept the existence, in Europe, of early Ice-Age Man, or of the beginnings of human life. For this he looks to the warmer zones which have yielded the Javan *Pithecanthropus* and the Chinese *Sinanthropus*. These beings he accepts definitely as ancestors of the human stock—he has nothing to say of complex genealogies of the Primates, with truncated branches ending in these and similar abortive forms.

The second chapter deals with the 'Primitive Formation of Man', and traces the development of the human body from preceding forms. He sees no gap in the sequence *Pithecanthropus*—*Sinanthropus* 1—*Sinanthropus* 2—*Homo primigenius*. The Mauer jaw for him belongs to a middle-ice-age precursor of the fully developed *Primigenius* of Neandertal and his fellows, with whom he groups the Tabgha skull of Galilee and the Broken Hill skull of South Africa. All of these are outliers of a wide-spread human race, which, under climatic and other influences, is to develop into the varieties of *Homo sapiens*. Clearly such a scheme allows no room for an anachronistic *Homo sapiens* in the Acheulean period. This part of the book is packed with information, impossible to summarize—for it is itself a summary, one of the best and fullest that has come our way, of the knowledge of the facts of human development acquired down to the date of publication.

A valuable part of this section of the book is its definitive recognition of a triple division of *Homo sapiens* in late glacial times—the Löss Man of Brünn and Predmost, the Cro-Magnon Man, and the Grimaldi Man—and the description of the physical characters of each : together with a reconstruction of the interplay of these three human groups, and of their respective contributions to civilization.

At p. 90 the real subject of the book, the 'Awakening and Development of the Human Soul', begins. At the beginning of the work the author makes a claim that here he breaks new ground : a claim to which at first sight, with the works of such writers as Mainage and Luquet in remembrance, we were inclined to affix a query. But as we read on, we removed the query : for although old ground is inevitably traversed, the author finds something new at almost every step. There is no book known to us in which the subject is treated in exactly the same way, or in which so many relevant facts are correlated.

Once more, the reviewer is at a loss. The egg is so full of meat that it cannot be adequately compressed into a review. First we read of the technical awakening of humanity, under the twofold stimulus of warfare with formidable beasts and of food-acquisition : here incidentally we note an interesting analogy drawn between the outline of the primitive *coup de poing* and of the human hand. At the same time deeper problems suggest themselves—death, and the hypothetical necessities of the *lebende Leichnam* with which the awakening man finds himself embarrassed.

Next, game becomes increasingly difficult to procure, and a hunting-magic, culminating in the marvellous cave-paintings, gradually develops. Again, nature has implanted a desire to multiply and to perpetuate the species ; and erotic magic, expressing itself in personal ornaments, comes into existence. There is, of course, nothing new in either of these theses ; but they are here treated with unusual thoroughness, especially in their initial manifestations. In a shorter chapter, dealing with the dance and its magic, the



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cult of the dead, and the interpretation of the Spanish Capsian paintings, the book leaves us on the threshold of the complex theologies to be developed in later times.

The book ends with a number of evolutionary and chronological tables, an adequate bibliography, and an analytical index. We could have wished to see this supplemented with an ordinary alphabetical index.

If someone would translate this book into English, he would provide our students with an introduction to the study of Palaeolithic Man which it would be difficult to supersede.\*

R. A. S. MACALISTER.

**MORE MOVES ON AN EASTERN CHEQUERBOARD.** By SIR HARRY LUKE. *Lovat Dickson and Thompson, 1935. pp. x, 270 and 24 plates. 12s 6d.*

The author of these readable chapters has travelled widely in an official capacity, and watched the light and shade of post-war events passing over the eastern landscape. His erudition is great, but, we hasten to add, not overwhelming, either for himself or for the reader. It consists of the sort of knowledge one needs when travelling in 'foreign parts', but which, alas, one usually only acquires after one's return home. As one reads, one keeps saying 'I wish I had known that when I was there'; and one envies him also his knowledge of languages. Historically the essays are valuable because they often cast side-lights upon recent events which are probably unrecorded elsewhere, such as those in Georgia and Armenia before the Soviet regime was established, and in Lemnos during the war. We are grateful to the author for some good stories and some unforgettable pictures; the desert bath-house of Quseir Amra with its 'charming, and even suggestive, painting', and the 'grey elongated figure' of the dead Bedu; the episcopal gin-palace of Lemnos; the sceptical Ossete (p. 93); the photograph of a 'Cypriote priest expressing olive-oil' (opp. p. 190), and of Kiamil Pasha in Cyprus (opp. p. 186); and the tale (quoted) of Mohammed's maternal aunt searching his holy head for lice. It is good to find that Sir Harry Luke appreciated the difference between Armenians 'of the Diaspora', living under Turkish or British rule and 'the inhabitants of the independent Armenia'. One gathers that he preferred the latter, as did the reviewer, who found them both capable and charming. Their resemblance in some respects to the Jews, which he remarked, is in accordance with anthropological views; for the hooked nose, so characteristic of the ancient Hittites, is an Anatolian rather than a Palestinian feature.

Those who want their history and archaeology presented in a palatable form will find it here, together with many (dare we say welcome?) antidotes! O.G.S.C.

**CHAMBER TOMBS AT MYCENAE.** By A. J. B. WACE. (*Archaeologia*, LXXXII). *Oxford: for the Society of Antiquaries, 1932. pp. XIII, 242, frontispiece, 57 plates (9 in colour), 50 plans and illustrations. £5 5s.*

This finishes the publication of the excavations at Mycenae of the British School at Athens under Professor Wace in 1920-1923. The Society of Antiquaries undertook the publication, and the rich illustration is due to the generosity of Miss M. B. Wright, of Washington. Twenty-four Bronze Age tombs were found, distributed in the 'Third Kilometre' and Kalkani cemeteries. Tsountas, who excavated many tombs at Mycenae, supposed that these were grouped in clan or family cemeteries, and his conjecture is strongly supported. The tombs range in date from the First Late Helladic period to

\* We commend this suggestion to publishers. We know of no recent book in English which covers the same ground.—EDITOR.



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the very end of the Third Late Helladic (from c. 1600 to c. 1100 B.C.), and supply a means of studying the funerary habits, pottery and other utensils, personal treasures, and even racial type of the ruling class in the period of Mycenae's greatest power. For the style of the tombs and of their contents—even though the richer portion of the latter has mostly to be inferred—show that they were the resting places of wealthy families, and indeed Professor Wace suggests that these underground chambers were the lesser archetypal type of the royal beehive-tombs: these are not the humble burials of a subject population. Thus while the book must be closely studied by specialists, its interest is much wider and more general, and some account of its main conclusions will be welcome here.

The chamber-tomb may, it is surmised, have been developed from the Early Helladic rock-shelter, and itself have given rise to the tholos-tomb. The Mycenae tombs were hollowed out of the soft rock of the hill-side, and in constructional details followed natural demands. Thus there is no rule for orientation; the plan, usually roughly rectangular, is not invariably so, and was probably always dictated by convenience and owes nothing to the inspiration of house-plans. The height is very variable, and conditioned by circumstance; and neither proportions nor size nor chamber-plan nor width of doorway can be used as a criterion of date. But the type of dromos—a tunnel cut in the rock without reinforcement—changes, as at Asine, from the rather wide and short passages of L.H. I and II to the narrow, long ones, almost wedge-shaped in section, of L.H. III. And other, though not constant, features of the dromoi can be used as evidence of date.

\* It is amply proved that the tombs remained in use, as family vaults, from L.H. I to the very end of L.H. III. The dromos, filled in at the last burial, would be dug out (its entrance was presumably indicated by a 'marker'), not always necessarily to its whole depth; and the bones of previous interments placed in pits in the dromos; this is nicely illustrated by the observation that sherds found in one such pit belonged to pots most of whose fragments occurred with early interments in the chamber. Less often these pits were in the chamber itself. But sometimes the bones were swept with scant ceremony into the dromos or into a corner. It seems, however, possible that usage was refined with time, and that the remains of previous interments were covered with a layer of earth, above which the newly interred corpse was laid. Sometimes, indeed, the fall of soft rock from the roof anticipated this attention. The dead were, apparently, clothed: a man might wear a helmet adorned with boar's tusks, the women's clothes had gold rosettes. Rock-cut benches played a part in ritual. There was no embalming, nor, of course, cremation: but the tomb was fumigated. With the dead were placed objects of daily use: valuables were sometimes removed when a tomb was reopened, as family property; the dead seem to have been thought to possess a kind of life only so long as there was flesh upon the bones: perhaps fear was a motive for walling them in. In the dromoi many kylix fragments were found; as the earth was shovelled in, it is inferred, a last farewell was drunk and the cups were shattered. There is little or no evidence for or against human sacrifices: but a hound might go with his master to the grave.

The evidence for this reconstruction (together with matter here omitted) is contained in the detailed account of the excavations (parts I and II), and is collected in the commentary (part III). The effect produced by Professor Wace's account is enhanced by the restraint with which his results are described: for example, among the contents of one tomb we read of a jug, succinctly described and compared with parallels: its position when found is added: *'Chamber, just below edge of bench at south end, as though*



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*it had just fallen off*'. Observation can go no further; nor, surely, discretion in recording it.

The pottery is fully described in part III; it is the first collection of complete vases from Mycenae, except for the few from the Shaft-Graves, yet published; over 350 were found, very valuable evidence for the evolution of ceramic style; this is particularly important for L.H. I and II, and this publication must be added to the corpus of works devoted to Late Helladic pottery. An interesting pot with human figures is included. The non-ceramic objects include some bronze and precious metals (though not in great quantity), gems, beads, etc.; all these are fully described and illustrated.

The racial problem arises firstly by reference to burial practices, which in the mainland differed from those of Crete: for example, the posture of the dead, perhaps a survival from Middle Helladic usage; the excessively rare and late use of coffins; and the use of a peculiar type of funerary alabastron; and secondly from a consideration of Professor Fürst's appendix on the skeletal remains, a summary of findings published elsewhere under the auspices of the University of Lund. After remarking that the two Middle Helladic skulls from graves below the Ramp House are Nordic rather than Mediterranean, and perhaps indicate that there was early a population in Greece of non-Mediterranean race (and perhaps the neolithic skull from Hageorgitika in Arcadia should be taken into consideration here), Professor Fürst says that from the skulls examined from the Kalkani cemetery it is impossible to determine whether a new racial element entered Mycenae during the Late Helladic period: but from one tomb a range of skulls dating from L.H. I or II to the later part of L.H. III was obtained; and these happen to give the same length-breadth index (74) in all cases where this measurement could be taken. This supports the view that the tomb remained in the use of one family throughout. It is possible, from the general results, to predicate the existence of two racial groups, with the dolichocranic element preponderating. But only 13 skulls were available for measurement, so that the results need to be used with caution. Yet comparison with Professor Koumoures's measurements of other L.H. skulls from Mycenae does not alter the picture presented by Professor Fürst: that of a mixed population, of which the men differed racially from the women, who were Mediterraneans; the men may have been of Nordic race, so far as the anthropological evidence shows.

The top of the Kalkani hill, now bare rock, was probably inhabited, since Early and Middle Helladic sherds occur as part of the soil in the dromoi: the site should be explored both for traces of occupation and for Early and Middle Helladic burials: 'some likely places have been noted, and we know that there are chamber tombs still to be excavated on the slope of the hill which forms the south bank of the ravine'.

W. L. CUTLER.

**THE SWEDISH CYPRUS EXPEDITIONS:** Finds and Results of the Excavations in Cyprus, 1927-1931. By EINAR GJERSTAD, JOHN LINDROS, ERIK SJOQVIST, ALFRED WESTHOLM. Vol. 1. pp. 578, 16 plans, 217 text-figs., 155 plates. Stockholm: *Swedish Cyprus Expedition*. 145 kr.

Cyprus, though exceptionally endowed with archaeological treasures, has long been extremely unlucky as regards their excavation and preservation. For many years the ancient cemeteries and sanctuaries of the island were the hunting ground of amateurs, and also unfortunately of tomb robbers. As a result there is hardly a museum of any importance which does not possess a collection of Cypriote antiquities, but antiquities that have been torn from their contexts and are in consequence archaeologically dumb.



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The Cesnola Collection, now in New York, showed what might be found, but the world has always cared more for museum specimens than for scientific archaeology. Although official circles were lukewarm after the British occupation, Ohnefalsch Richter did useful work and the Cyprus Exploration Fund began systematic excavations. The valuable researches of Myres proved that order could be introduced into Cypriote archaeology and that from it much important information could be won. Unfortunately his excellent example was not followed up by his fellow countrymen till recent times, which have seen a welcome revival of official interest in the antiquities of the island. Thus the Swedish Expedition, which under the direction of Dr Einar Gjerstad worked from 1927 to 1931, was the first to conduct really modern scientific excavations on a sufficiently adequate scale in Cyprus. That the first volume of the results has been able to appear so soon is due to the systematic organization of its operations and the provision of proper facilities for field and for museum work. The book is well got up, and it is well printed and equipped with all the necessary plans, sections, and text-figures, and the plates are admirable. The material found ranges from the Neolithic Period through the Bronze and Early Iron Ages, the classical and Hellenistic periods down to Roman times. The Expedition excavated and examined with scrupulous care temples, tombs, settlements, fortresses, a theatre at Soli, and a most interesting palace at Vouni. The principles and methods of the excavation as described on p. xv were fully in accord with all modern scientific requirements, which are unfortunately not always followed, even by British explorers. This volume is a record of the work done at nine main sites. At Petra tou Limniti there was a pre-neolithic settlement. At Lapithos, in addition to a neolithic site, many tombs were explored and the full publication of this more recent work emphasizes the delay in publishing the results of the British excavations there in 1913. Here, as in the later sections dealing with other excavations, full details are given of the stratification, of the architecture, and of the finds which are also set out in sequence-tables according to the strata. By these means the relative chronology of the site is determined. At Kythrea another neolithic settlement was found with interesting house-plans. At Ayios Iakovos tombs and sanctuaries of the Bronze and Iron Ages were explored. At Nitovikla a fortress architecturally important was cleared and near it a tumulus cemetery at Paleoskoutella, a type hitherto unknown in Cyprus. In the same neighbourhood Kountoura Trachonia yielded Hellenistic tombs. At Trachonas another important architectural find was a built tomb of stone dated to the middle of the first archaic period and similar in style and construction to some found at Tamassos by Ohnefalsch Richter. Last, but by no means least, comes Enkomi (a site which will always be a reproach to the good name of the British Museum) where at last scientific excavation has given us the information so badly needed about one of the most important cemeteries of the late Bronze Age which has been so badly mauled by unscientific or illicit digging. The detailed accounts of the tombs and of the stratification of the burials they contained and the full catalogue of the finds from each shows how much priceless information was missed by the earlier excavators. Now the archaeological facts given by the Swedish Expedition about the cemetery, together with the contemporary material from Ayios Iakovos, throw much needed light on the previous finds. The discoveries too of Dr. Schaeffer at Ras Shamra on the Syrian coast opposite also fit most opportunely into the same context. The chronological sequence which has thus been made out will be invaluable for all students of Eastern Mediterranean archaeology, especially in view of the Syrian and the Aegean connexions which will illuminate the extension of Mycenaean influence in the Levant. The excavators' conclusions on these points will be eagerly



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awaited and if the succeeding volumes maintain the same high standard they may well claim to have set Cypriote archaeology on an entirely new footing, its greatest advance since Myres' catalogues of the Cyprus Museum and of the Cesnola Collection. So to Dr Gjerstad and his collaborators our warmest thanks and heartiest congratulations on a fine piece of work so scientifically carried out and so adequately published.

TROLDEBJERG, EN BYMAESSIG BEBYGGELSE FRA DANMARKS YNGRE STENALDER. By J. WINTHIER. *Rudkøbing*, 1935.

Attention was drawn in *ANTIQUITY* for June 1934, pp. 206-7, to the importance of Herr Winther's excavations on a settlement-site of megalithic folk of the early passage-grave period at Troldebjerg, on the Danish island of Langeland. The hope, there expressed, of a fully illustrated report has been realized in the well-produced publication before us. The work is made available to a wider public by the provision of a German summary.

The excavations extended over four seasons (1930-33) and occupied a whole year in the field, but Herr Winther must feel satisfied that the results have more than justified his labours. The masses of sherds recovered show that we have to deal with a settlement of people living at the height of the 'grand style' of the northern megalithic pottery; quantities are illustrated by first-class half-tones and will be found useful for comparative purposes. But the real interest of the excavations lies in the light they throw on the economy, the houses, and even the religion of a society, most of our previous knowledge about which has been obtained from the excavation of graves.

Evidence for agriculture is provided by impressions of grains on potsherds, identified by Dr Knud Jessen as belonging to wheat and emmer (*Triticum dicoccum*); sickles occurred in two forms, the single-piece type being represented by a broken crescentic specimen, and the multiple type by five sickle flakes. Remains of domestic animals (ox, pig, sheep and dog) were plentiful, but hunting evidently played a very minor role.

Not the least interesting feature of the excavation was the discovery that two radically different house-forms—an approximately circular form with one entrance, and a rectangular form—were apparently inhabited at the same period, judging from the material removed from their floors. The circular (or horseshoe) houses were not very large, varying between 4 and 6 metres in diameter; traces of an outer covering of burnt clay were found in one instance. The rectangular houses were of the utmost interest, consisting of a row of three placed end to end at a distance of approximately a metre apart; the houses were placed on slightly different axes, though forming a single complex, the total length of which was 71 metres (233 feet). The houses seem to have had solid wooden walls to the west (presumably the quarter from which the prevailing winds blew), represented today by slots in the old ground surface bordered on either side by rows of small boulders. The central roof gable was supported by a row of substantial wooden uprights set into stone-packed post-holes, running the length of each hut parallel with the west wall. The eastern wall, indicated by a row of smaller post-holes at the northern ends of the huts, was evidently lower than the western and only extended for part of the length of the house, the roof extending pent-wise to the ground for the southern forepart of each house. In each case hearths and traces of occupation were confined to the forepart of the house, suggesting that the northern ends were used to house domestic animals, as in many peasant houses of today.

Some rather fanciful passages under the head of religion should not be allowed to

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obscure the interest of two finds pointing without doubt to some form of axe cult. On the floor of the slot for the western wall of the central of the three rectangular houses an isolated thick-butted flint axe was found, suggesting comparisons with 'thunder-bolt' beliefs current among peasants up to the present day. Secondly a 'votive offering', consisting of a similar flint axe set butt-downwards in the ground and accompanied by a small pot, was discovered underlying a spread of pebbles inside one of the circular huts.

J. G. D. CLARK.

A BOOK OF ANCIENT HISTORY FROM MINOS TO CONSTANTINE. By DINA PORTWAY DOBSON. *Nelson and Sons*, 1934. pp. 173, with illustrations. 2s.

This attractive little volume is suitable for junior pupils in secondary schools or older pupils in primary schools. It tells in very simple language the story of Greece and Rome from the Minoan civilization to the reign of Constantine. The information is up-to-date, and the choice of subjects is well-calculated to give to young children a right idea of ancient civilization, whether it is studied for itself or as an introduction to later history. As far as possible, the method of study suggested is practical, based on drawing and modelling, and at the end of each chapter a few exercises are suggested to make clear points raised in the text. The book is profusely illustrated—with six pictures in colour (all except one—an unfortunate exception—representing ancient works of art, either vases or wall-paintings), over fifty in black and white (of wide interest and range), and ten maps.

R. C. CARRINGTON.

THE ORIGINS OF THE INSULAE AT OSTIA. By PHILIP HARSH. *Memoirs of the American Academy in Rome*, 1935, XII, 9-66, pl. 1-3.

This study makes a wide survey of Mediterranean houses in ancient times in an effort to trace the origins of the large blocks of tenements that have been uncovered at Ostia. In an introductory section on 'Significant Developments at Pompeii', the author examines several types of house at Pompeii, that either have been taken or might be taken as prototypes of Ostian houses. He attacks the theory, put forward in *ANTIQUITY*, (June 1933, VII, 133-152), that a logical development can be traced between the old atrium-house and the 'House of Diana' or the 'House of the Round Temple' at Ostia. At the same time he picks out certain Pompeian houses that, in his view, offer closer analogies. The main body of the paper treats of the houses at Ostia and similar houses found elsewhere. Three kinds are distinguished: (1) a type with an open cortile, surrounded by shallow corridors, at the centre; (2) a type with an open cortile without corridors; (3) a type, without a central cortile, composed mostly of rows of shops, back to back. The first type, in the author's view, was an adaptation of the Greek 'peristyle' house to the needs of life in large cities, while the other two types might have been evolved independently.

R. C. CARRINGTON.



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